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THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

T. LEWIS, Sec.R.A.S.,
H. P. HOLLIS, B.A., F.R.A.S.

VOL. XXX.

LONDON:
TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.
1907.

147132

ALERE FLAMMAM.

PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.

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ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 378. 1907. Vol. XXX.

INTRODUCTION.

The present 'Companion' resembles those of previous years except for the alterations noted below.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Maw for kindly supplying a number of observations of Double Stars.

M. Lœwy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. Owing to the continued increase in the number of known Variables—there are now 574 in M. Lœwy's list—it has been found necessary to make some departure from our former system. The complete list of Stars with their places is now not given, and the dates of maxima and minima of long-period Variables are shown in a somewhat different form. In accordance with request, these Ephemerides are given in *Greenwich Mean Astronomical Time, counting from noon to noon* and not from midnight to midnight as in recent years, and the range of magnitude is added at the top of the column. The occultations of stars by the Moon, which are also increased in number by the addition of stars fainter than 6·5, the phenomena of Jupiter's Satellites, and the Ephemerides of the Satellites of the other planets are from the 'Nautical Almanac.' No diagram of the orbits of Saturn's Satellites is given, as their plane passes through the Earth in 1907.

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day from mean noon on January 1. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign + indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used, and the astronomical day is reckoned from noon to noon.

Date.	Day of Year.	Fr. of Year.	Sun Sets.	Sun Rises.	☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
1907.			h m	h m	° ′	h m s	h m s	d h m
Jan. 7	7	·016	4 6	20 6	22 29 S	0 6 3	19 3 36	Jan. ☾ 7 2 47
14	14	·036	4 15	20 2	21 27	8 55	19 31 12	● 13 17 57
21	21	·055	4 26	19 55	20 6	11 15	19 58 48	☽ 20 20 42
28	28	·074	4 39	19 46	18 25	12 58	20 26 24	○ 29 1 45
Feb. 4	35	·093	4 51	19 35	16 29	14 1	20 54 0	Feb. ☾ 5 12 52
11	42	·112	5 3	19 23	14 18	14 25	21 21 36	● 12 5 43
18	49	·131	5 17	19 11	11 56	14 11	21 49 12	☽ 19 16 35
25	56	·151	5 29	18 55	9 24	13 23	22 16 48	○ 27 18 23
Mar. 4	63	·170	5 41	18 41	6 46	12 5	22 44 23	March. ☾ 6 20 42
11	70	·189	5 54	18 25	4 3	10 25	23 11 59	● 13 18 5
18	77	·208	6 6	18 9	1 18 S	8 29	23 39 35	☽ 21 13 10
25	84	·227	6 17	17 53	1 28 N	6 23	0 7 11	○ 29 7 44
April 1	91	·246	6 29	17 37	4 12	4 14	0 34 47	April. ☾ 5 3 20
8	98	·266	6 40	17 22	6 52	2 10	1 2 23	● 12 7 6
15	105	·285	6 52	17 7	9 27	0 17	1 29 59	☽ 20 8 38
22	112	·304	7 4	16 52	11 54	23 58 41	1 57 35	○ 27 18 5
29	119	·323	7 15	16 37	14 11	57 25	2 25 10	
May 6	126	·342	7 27	16 25	16 17	56 35	2 52 46	May. ☾ 4 9 53
13	133	·361	7 38	16 14	18 10	56 12	3 20 22	● 11 20 59
20	140	·381	7 48	16 3	19 48	56 18	3 47 58	☽ 20 1 27
27	147	·400	7 58	15 55	21 9	56 49	4 15 34	○ 27 2 18
June 3	154	·419	8 6	15 49	22 12	57 45	4 43 10	June. ☾ 2 17 20
10	161	·438	8 12	15 45	22 57	58 59	5 10 46	● 10 11 50
17	168	·457	8 16	15 44	23 22	0 0 26	5 38 22	☽ 18 14 55
24	175	·476	8 19	15 45	23 26	1 57	6 5 58	○ 25 9 27
July 1	182	·496	8 18	15 49	23 11	3 23	6 33 33	July. ☾ 2 2 34
8	189	·515	8 16	15 54	22 35	4 39	7 1 9	● 10 3 17
15	196	·534	8 10	16 3	21 41	5 38	7 28 45	☽ 18 1 12
22	203	·553	8 2	16 11	20 28	6 11	7 56 21	○ 24 16 29
29	210	·572	7 53	16 21	18 59	6 17	8 23 57	☾ 31 14 25
Aug. 5	217	·591	7 42	16 32	17 14	5 54	8 51 33	August. ● 8 18 36
12	224	·611	7 30	16 42	15 15	5 3	9 19 9	☽ 16 9 5
19	231	·630	7 15	16 53	13 5	3 43	9 46 45	○ 23 0 15
26	238	·649	7 1	17 5	10 44	1 59	10 14 21	☾ 30 5 28
Sept. 2	245	·668	6 46	17 16	8 15	23 59 55	10 41 56	Sept. ● 7 9 4
9	252	·687	6 30	17 27	5 40	57 37	11 9 32	☽ 14 15 40
16	259	·706	6 14	17 36	3 0	55 11	11 37 8	○ 21 9 34
23	266	·726	5 58	17 48	0 16 N	52 42	12 4 44	☾ 28 23 37
30	273	·745	5 42	18 0	2 27 S	50 19	12 32 20	
Oct. 7	280	·764	5 26	18 12	5 10	48 8	12 59 56	Oct. ● 6 22 21
14	287	·783	5 10	18 24	7 49	46 17	13 27 32	☽ 13 22 2
21	294	·802	4 55	18 36	10 23	44 51	13 55 8	○ 20 21 16
28	301	·821	4 41	18 48	12 50	43 56	14 22 43	☾ 28 19 51
Nov. 4	308	·841	4 28	19 1	15 7	43 39	14 50 19	Nov. ● 5 10 39
11	315	·860	4 17	19 13	17 11	44 1	15 17 55	☽ 12 5 14
18	322	·879	4 6	19 25	19 2	45 4	15 45 31	○ 19 12 4
25	329	·898	3 58	19 36	20 35	46 47	16 13 7	☾ 27 16 21
Dec. 2	336	·917	3 53	19 47	21 50	49 9	16 40 43	Dec. ● 4 22 22
9	343	·936	3 50	19 54	22 45	52 2	17 8 18	☽ 11 14 16
16	350	·956	3 49	20 2	23 17	55 17	17 35 55	○ 19 5 55
23	357	·975	3 52	20 6	23 27	58 44	18 3 31	☾ 27 11 10
30	364	·994	3 57	20 8	23 14 S	0 2 12	18 31 6	

For Moon's phases: ● signifies New; ☽ First Quarter; ○ Full; ☾ Last Quarter.

JANUARY.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	5 49	13 50	19 17 N	+61·4 E.
2	W	6 52	14 38	17 1	+49·3 E.
3	Th	7 58	15 26	13 57	+37·2 E.
4	F	9 7	16 12	10 12	+25·0 E.
5	S	10 16	16 59	5 55	+12·9 E.
6	**S**	11 28	17 46	1 17 N	+ 0·7 E.
7	M	12 43	18 34	3 33 S	−11·4 E.
8	Tu	13 59	19 25	8 19	−23·6 E.
9	W	15 20	20 19	12 47	−35·8 E.
10	Th	16 40	21 17	16 35	−47·9 E.
11	F	17 58	22 18	19 23	−60·1 E.
12	S	19 9	23 21	20 53	−72·3 E.
13	**S**	sets	……	20 52	−84·5 E.
14	M	4 43	0 24	19 23	+83·3 M.
15	Tu	6 1	1 25	16 38	+71·1 M.
16	W	7 19	2 22	12 56	+58·9 M.
17	Th	8 37	3 14	8 38	+46·7 M.
18	F	9 50	4 4	4 3 S	+34·6 M.
19	S	11 2	4 50	0 34 N	+22·4 M.
20	**S**	12 10	5 35	5 2	+10·2 M.
21	M	13 17	6 18	9 11	− 1·9 M.
22	Tu	14 22	7 2	12 53	−14·1 M.
23	W	15 26	7 47	16 2	−26·2 M.
24	Th	16 27	8 32	18 30	−38·4 M.
25	F	17 25	9 19	20 12	−50·5 M.
26	S	18 17	10 8	21 0	−62·7 M.
27	**S**	19 3	10 57	20 51	−74·8 M.
	M	rises	11 46	19 44	−86·9 M.
		4 43	12 35	17 41	+80·9 E.

FEBRUARY.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	8 8	14 57	6 57 N	+44·5 E.
2	S	9 19	15 44	2 22 N	+32·4 E.
3	**S**	10 34	16 31	2 24 S	+20·3 E.
4	M	11 47	17 20	7 8	+ 8·1 E.
5	Tu	13 3	18 12	11 35	− 4·1 E.
6	W	14 21	19 6	15 30	−16·2 E.
7	Th	15 38	20 3	18 33	−28·4 E.
8	F	16 49	21 3	20 29	−40·6 E.
9	S	17 52	22 5	21 4	−52·8 E.
10	**S**	18 45	23 6	20 12	−65·0 E.
11	M	sets	……	18 0	−77·2 E.
12	Tu	4 50	0 4	14 40	−89·4 E.
13	W	6 7	0 59	10 33	+78·4 M.
14	Th	7 26	1 51	5 58	+66·2 M.
15	F	8 39	2 40	1 13 S	+54·0 M.
16	S	9 52	3 26	3 26 N	+41·9 M.
17	**S**	11 1	4 12	7 49	+29·7 M.
18	M	12 8	4 56	11 46	+17·5 M.
19	Tu	13 13	5 41	15 9	+ 5·3 M.
20	W	14 17	6 27	17 52	− 6·9 M.
21	Th	15 15	7 13	19 49	−19·0 M.
22	F	16 10	8 1	20 54	−31·2 M.
23	S	16 59	8 50	21 3	−43·3 M.
24	**S**	17 41	9 39	20 14	−55·5 M.
25	M	18 17	10 28	18 27	−67·6 M.
26	Tu	18 48	11 17	15 46	−79·8 M.
27	W	rises	12 5	12 17	+88·1 E.
28	Th	5 54	12 53	8 8 N	+75·9 E.

MARCH.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	7 7	13 41	3 32 N	+63·8 E.
2	S	8 22	14 29	1 19 S	+51·6 E.
3	**S**	9 37	15 18	6 10	+39·5 E.
4	M	10 54	16 8	10 45	+27·3 E.
5	Tu	12 12	17 2	14 48	+15·2 E.
6	W	13 26	17 57	18 3	+ 3·0 E.
7	Th	14 39	18 55	20 14	− 9·2 E.
8	F	15 44	19 54	21 10	−21·4 E.
9	S	16 39	20 54	20 43	−33·6 E.
10	**S**	17 22	21 51	18 58	−45·8 E.
11	M	17 58	22 46	16 3	−58·0 E.
12	Tu	18 28	23 39	12 14	−70·2 E.
13	W	sets	……	7 49	−82·4 E.
14	Th	6 17	0 29	3 4 S	+85·4 M.
15	F	7 31	1 16	1 43 N	+73·2 M.
16	S	8 42	2 2	6 18	+61·0 M.
17	**S**	9 51	2 48	10 31	+48·8 M.
18	M	10 59	3 33	14 11	+36·6 M.
19	Tu	12 3	4 19	17 12	+24·4 M.
20	W	13 5	5 6	19 26	+12·2 M.
21	Th	14 3	5 53	20 49	+ 0·0 M.
22	F	14 53	6 42	21 17	−12·2 M.
23	S	15 38	7 30	20 47	−24·4 M.
24	**S**	16 16	8 19	19 20	−36·5 M.
25	M	16 48	9 8	16 56	−48·7 M.
26	Tu	17 15	9 57	13 42	−60·9 M.
27	W	17 40	10 45	9 43	−73·0 M.
28	Th	rises	11 33	5 10	−85·2 M.
29	F	6 3	12 21	0 15 N	+82·7 E.

APRIL.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	9 59	14 56	14 1 S	+46·2 E.
2	Tu	11 18	15 52	17 35	+34·0 E.
3	W	12 33	16 50	20 4	+21·9 E.
4	Th	13 41	17 49	21 18	+ 9·7 E.
5	F	14 36	18 48	21 10	− 2·5 E.
6	S	15 24	19 46	19 42	−14·7 E.
7	**S**	16 0	20 40	17 5	−26·9 E.
8	M	16 30	21 32	13 32	−39·1 E.
9	Tu	16 56	22 22	9 18	−51·3 E.
10	W	17 20	23 9	4 40 S	−63·6 E.
11	Th	17 38	23 55	0 6 N	−75·8 E.
12	F	sets	……	4 48	−88·0 E.
13	S	7 35	0 41	9 12	+79·8 M.
14	**S**	8 43	1 26	13 8	+67·5 M.
15	M	9 50	2 12	16 27	+55·3 M.
16	Tu	10 53	2 59	19 0	+43·1 M.
17	W	11 53	3 46	20 43	+30·9 M.
18	Th	12 47	4 34	21 30	+18·7 M.
19	F	13 35	5 23	21 19	+ 6·5 M.
20	S	14 14	6 11	20 12	− 5·8 M.
21	**S**	14 48	7 0	18 8	−18·0 M.
22	M	15 17	7 47	15 12	−30·1 M.
23	Tu	15 42	8 35	11 30	−42·3 M.
24	W	16 4	9 22	7 9	−54·5 M.
25	Th	16 27	10 10	2 19 N	−66·7 M.
26	F	16 50	11 0	2 47 S	−78·9 M.
27	S	rises	11 51	7 52	+89·0 E.
28	**S**	7 37	12 45	12 37	+76·8 E.
29	M	8 58	13 42	16 39	+64·6 E.

MAY.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	11 32	15 42	21 20 S	+40·3 E.
2	Th	12 36	16 43	21 34	+28·1 E.
3	F	13 25	17 41	20 24	+15·9 E.
4	S	14 4	18 37	18 0	+ 3·7 E.
5	**S**	14 36	19 30	14 37	− 8·5 E.
6	M	15 3	20 19	10 31	−20·7 E.
7	Tu	15 26	21 7	5 59	−33·0 E.
8	W	15 46	21 52	1 15 S	−45·2 E.
9	Th	16 5	22 37	3 27 N	−57·4 E.
10	F	16 26	23 22	7 57	−69·7 E.
11	S	sets	……	12 4	−81·9 E.
12	**S**	7 37	0 7	15 36	+85·9 M.
13	M	8 43	0 53	18 27	+73·6 M.
14	Tu	9 44	1 40	20 27	+61·4 M.
15	W	10 41	2 28	21 34	+49·2 M.
16	Th	11 32	3 17	21 42	+36·9 M.
17	F	12 13	4 5	20 53	+24·7 M.
18	S	12 49	4 53	19 8	+12·5 M.
19	**S**	13 21	5 41	16 30	+ 0·2 M.
20	M	13 45	6 27	13 7	−12·0 M.
21	Tu	14 9	7 14	9 3	−24·2 M.
22	W	14 30	8 0	4 26 N	−36·4 M.
23	Th	14 53	8 48	0 32 S	−48·6 M.
24	F	15 13	9 37	5 38	−60·8 M.
25	S	15 39	10 29	10 36	−73·0 M.
26	**S**	rises	11 25	15 4	−85·1 M.
27	M	7 52	12 24	18 39	+82·7 E.
28	Tu	9 12	13 26	20 59	+70·5 E.
29	W	10 23	14 29	21 49	+58·3 E.
30	Th	11 20	15 32	21 6	+46·1 E.

JUNE.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	12 40	17 26	15 46 S	+21·7 E.
2	**S**	13 8	18 17	11 45	+ 9·5 E.
3	M	13 33	19 5	7 14	− 2·7 E.
4	Tu	13 52	19 51	2 30 S	−14·9 E.
5	W	14 13	20 36	2 15 N	−27·2 E.
6	Th	14 33	21 20	6 48	−39·4 E.
7	F	14 56	22 5	11 1	−51·6 E.
8	S	15 19	22 50	14 43	−63·9 E.
9	**S**	15 46	23 37	17 46	−76·1 E.
10	M	sets	……	20 3	−88·4 E.
11	Tu	8 34	0 24	21 26	+79·4 M.
12	W	9 29	1 13	21 52	+67·1 M.
13	Th	10 13	2 1	21 19	+54·9 M.
14	F	10 51	2 50	19 50	+42·6 M.
15	S	11 22	3 37	17 28	+30·4 M.
16	**S**	11 49	4 23	14 20	+18·2 M.
17	M	12 13	5 9	10 31	+ 5·9 M.
18	Tu	12 35	5 54	6 10	− 6·3 M.
19	W	12 57	6 39	1 26 N	−18·5 M.
20	Th	13 16	7 26	3 32 S	−30·7 M.
21	F	13 41	8 16	8 29	−42·9 M.
22	S	14 6	9 8	13 9	−55·1 M.
23	**S**	14 40	10 5	17 9	−67·3 M.
24	M	15 21	11 5	20 7	−79·5 M.
25	Tu	rises	12 9	21 41	+88·3 E.
26	W	9 6	13 13	21 39	+76·1 E.
27	Th	9 59	14 16	20 4	+63·9 E.
28	F	10 39	15 15	17 8	+51·7 E.
29	S	11 12	16 10	13 13	+39·5 E.
30	**S**	11 37	17 1	8 42 S	+27·3 E.

JULY.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	12 0	17 49	3 53 S	+15·1 E.
2	Tu	12 20	18 34	0 58 N	+ 2·9 E.
3	W	12 40	19 19	5 38	− 9·3 E.
4	Th	13 1	20 3	9 58	−21·6 E.
5	F	13 24	20 48	13 49	−33·8 E.
6	S	13 50	21 34	17 3	−46·1 E.
7	**S**	14 22	22 21	19 32	−58·3 E.
8	M	14 59	23 10	21 9	−70·6 E.
9	Tu	15 43	23 58	21 51	−82·8 E.
10	W	sets	……	21 34	+84·9 M.
11	Th	8 53	0 47	20 19	+72·7 M.
12	F	9 27	1 34	18 10	+60·4 M.
13	S	9 55	2 21	15 13	+48·2 M.
14	**S**	10 19	3 7	11 35	+35·9 M.
15	M	10 40	3 52	7 24	+23·7 M.
16	Tu	11 2	4 36	2 49 N	+11·5 M.
17	W	11 22	5 21	1 59 S	− 0·7 M.
18	Th	11 42	6 8	6 50	−13·0 M.
19	F	12 6	6 57	11 29	−25·2 M.
20	S	12 35	7 50	15 39	−37·4 M.
21	**S**	13 11	8 47	19 0	−49·6 M.
22	M	13 59	9 48	21 10	−61·8 M.
23	Tu	14 58	10 51	21 52	−74·0 M.
24	W	rises	11 55	20 58	−86·2 M.
25	Th	8 31	12 57	18 34	+81·7 E.
26	F	9 9	13 56	14 57	+69·5 E.
27	S	9 37	14 50	10 31	+57·3 E.
28	**S**	10 3	15 41	5 37	+45·1 E.
29	M	10 24	16 29	0 37 S	+32·9 E.
30	Tu	10 44	17 15	4 16 N	+20·7 E.

AUGUST.					
Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	11 29	18 46	12 51 N	− 3·8 E.
2	F	11 54	19 31	16 17	−16·0 E.
3	S	12 23	20 18	18 58	−28·2 E.
4	**S**	12 58	21 6	20 50	−40·5 E.
5	M	13 41	21 54	21 46	−52·7 E.
6	Tu	14 49	22 43	21 44	−64·9 E.
7	W	15 26	23 31	20 43	−77·2 E.
8	Th	sets	……	18 46	−89·4 E.
9	F	7 59	0 19	15 59	+78·3 M.
10	S	8 24	1 5	12 28	+66·1 M.
11	**S**	8 48	1 50	8 22	+53·8 M.
12	M	9 7	2 35	3 51 N	+41·6 M.
13	Tu	9 27	3 20	0 54 S	+29·4 M.
14	W	9 47	4 6	5 42	+17·1 M.
15	Th	10 10	4 53	10 21	+ 4·9 M.
16	F	10 36	5 43	14 34	− 7·3 M.
17	S	11 8	6 36	18 6	−19·5 M.
18	**S**	11 49	7 33	20 37	−31·7 M.
19	M	12 41	8 34	21 50	−43·9 M.
20	Tu	13 45	9 36	21 34	−56·1 M.
21	W	15 1	10 38	19 47	−68·3 M.
22	Th	rises	11 38	16 39	−80·5 M.
23	F	7 36	12 35	12 29	+87·4 E.
24	S	8 2	13 28	7 40	+75·2 E.
25	**S**	8 27	14 18	2 33 S	+63·0 E.
26	M	8 46	15 6	2 33 N	+50·8 E.
27	Tu	9 8	15 53	7 21	+38·6 E.
28	W	9 31	16 40	11 42	+26·4 E.
29	Th	9 54	17 26	15 25	+14·2 E.
30	F	10 22	18 13	18 23	+ 2·0 E.

SEPTEMBER.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	11 36	19 49	21 42 N	−22·4 E.
2	M	12 22	20 38	21 56	−34·6 E.
3	Tu	13 17	21 26	21 11	−46·9 E.
4	W	14 17	22 14	19 28	−59·1 E.
5	Th	15 22	23 1	16 52	−71·3 E.
6	F	16 29	23 47	13 29	−83·6 E.
7	S	sets	......	9 27	+84·2 M.
8	**S**	7 13	0 33	4 57	+72·0 M.
9	M	7 33	1 18	0 9 N	+59·7 M.
10	Tu	7 53	2 4	4 44 S	+47·5 M.
11	W	8 15	2 51	9 29	+35·3 M.
12	Th	8 38	3 40	13 50	+23·1 M.
13	F	9 8	4 32	17 31	+10·9 M.
14	S	9 45	5 27	20 16	− 1·4 M.
15	**S**	10 32	6 25	21 48	−13·6 M.
16	M	11 31	7 25	21 58	−25·7 M.
17	Tu	12 40	8 25	20 41	−37·9 M.
18	W	13 58	9 24	18 2	−50·1 M.
19	Th	15 18	10 21	14 16	−62·3 M.
20	F	16 40	11 14	9 41	−74·4 M.
21	S	rises	12 6	4 38 S	−86·6 M.
22	**S**	6 48	12 55	0 34 N	+81·2 E.
23	M	7 9	13 43	5 37	+69·1 E.
24	Tu	7 31	14 30	10 16	+56·9 E.
25	W	7 55	15 18	14 20	+44·7 E.
26	Th	8 21	16 5	17 39	+32·5 E.
27	F	8 53	16 54	20 7	+20·4 E.
28	S	9 29	17 42	21 38	+ 8·2 E.
	S	10 14	18 31	22 11	− 4·0 E.

OCTOBER.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	12 3	20 8	20 18 N	−28·4 E.
2	W	13 7	20 55	17 57	−40·6 E.
3	Th	14 13	21 41	14 46	−52·9 E.
4	F	15 23	22 27	10 53	−65·1 E.
5	S	16 34	23 13	6 26	−77·3 E.
6	**S**	17 46	23 59	1 36 N	−89·5 E.
7	M	sets	......	3 25 S	+78·3 M.
8	Tu	6 17	0 46	8 22	+66·1 M.
9	W	6 43	1 36	12 58	+53·8 M.
10	Th	7 10	2 28	16 56	+41·6 M.
11	F	7 45	3 23	19 59	+29·4 M.
12	S	8 27	4 20	21 49	+17·2 M.
13	**S**	9 22	5 19	22 18	+ 5·0 M.
14	M	10 28	6 19	21 21	− 7·2 M.
15	Tu	11 42	7 17	19 4	−19·3 M.
16	W	12 59	8 13	15 39	−31·5 M.
17	Th	14 19	9 6	11 21	−43·7 M.
18	F	15 38	9 57	6 29	−55·8 M.
19	S	16 55	10 46	1 20 S	−68·0 M.
20	**S**	rises	11 33	3 48 N	−80·1 M.
21	M	5 34	12 21	8 40	+87·7 E.
22	Tu	5 55	13 8	13 2	+75·6 E.
23	W	6 19	13 56	16 43	+63·4 E.
24	Th	6 49	14 45	19 34	+51·3 E.
25	F	7 23	15 34	21 28	+39·1 E.
26	S	8 5	16 23	22 22	+27·0 E.
27	**S**	8 53	17 12	22 15	+14·8 E.
28	M	9 49	18 0	21 9	+ 2·6 E.
29	Tu	10 51	18 47	19 6	− 9·6 E.

NOVEMBER.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	14 13	21 4	8 18 N	−46·2 E.
2	S	15 25	21 50	3 34 N	−58·4 E.
3	**S**	16 39	22 37	1 28 S	−70·6 E.
4	M	17 56	23 26	6 35	−82·8 E.
5	Tu	sets	......	11 29	+85·0 M.
6	W	5 9	0 18	15 52	+72·8 M.
7	Th	5 43	1 13	19 22	+60·6 M.
8	F	6 23	2 12	21 40	+48·4 M.
9	S	7 13	3 12	22 33	+36·2 M.
10	**S**	8 17	4 13	21 57	+24·1 M.
11	M	9 30	5 13	19 56	+11·9 M.
12	Tu	10 47	6 9	16 44	− 0·3 M.
13	W	12 5	7 3	12 38	−12·5 M.
14	Th	13 23	7 53	7 56	−24·6 M.
15	F	14 40	8 42	2 54 S	−36·8 M.
16	S	15 55	9 28	2 12 N	−48·9 M.
17	**S**	17 8	10 15	7 8	−61·1 M.
18	M	18 20	11 1	11 41	−73·2 M.
19	Tu	rises	11 49	15 37	−85·3 M.
20	W	4 48	12 37	18 48	+82·5 E.
21	Th	5 20	13 26	21 5	+70·4 E.
22	F	5 59	14 15	22 22	+58·3 E.
23	S	6 44	15 5	22 36	+46·1 E.
24	**S**	7 38	15 54	21 50	+34·0 E.
25	M	8 36	16 41	20 6	+21·8 E.
26	Tu	9 39	17 27	17 30	+ 9·7 E.
27	W	10 45	18 12	14 8	− 2·5 E.
28	Th	11 55	18 56	10 8	−14·7 E.
29	F	13 3	19 41	5 38	−26·9 E.

DECEMBER.

Day.		Moon.			
Month.	Week.	Rises.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	15 30	21 13	4 20 S	−51·2 E.
2	M	16 48	22 4	9 22	−63·4 E.
3	Tu	18 9	22 58	14 4	−75·6 E.
4	W	19 32	23 56	18 4	−87·8 E.
5	Th	sets	......	21 0	+80·0 M.
6	F	5 1	0 57	22 31	+67·8 M.
7	S	6 1	2 0	22 27	+55·6 M.
8	**S**	7 14	3 3	20 49	+43·4 M.
9	M	8 33	4 3	17 51	+31·3 M.
10	Tu	9 55	4 59	13 52	+19·1 M.
11	W	11 12	5 51	9 12	+ 6·9 M.
12	Th	12 28	6 40	4 12 S	− 5·2 M.
13	F	13 43	7 27	0 54 N	−17·4 M.
14	S	14 57	8 13	5 52	−29·5 M.
15	**S**	16 8	8 58	10 28	−41·7 M.
16	M	17 18	9 45	14 34	−53·8 M.
17	Tu	18 26	10 32	17 58	−65·9 M.
18	W	19 30	11 20	20 32	−78·1 M.
19	Th	rises	12 9	22 8	+89·8 E.
20	F	4 38	12 59	22 42	+77·7 E.
21	S	5 27	13 48	22 15	+65·5 E.
22	**S**	6 25	14 36	20 48	+53·4 E.
23	M	7 28	15 23	18 27	+41·3 E.
24	Tu	8 33	16 8	15 20	+29·1 E.
25	W	9 40	16 51	11 34	+17·0 E.
26	Th	10 47	17 35	7 17	+ 4·8 E.
27	F	11 55	18 18	2 37 N	− 7·3 E.
28	S	13 6	19 3	2 16 S	−19·5 E.
29	**S**	14 20	19 50	7 13	−31·7 E.

Radiant-points of the principal Meteoric Showers of the Year.

By W. F. Denning.

Date.	Radiant.		Meteors.	Date.	Radiant.		Meteors.
	α °	δ °			α °	δ °	
Jan. 2-3..	**230**	**+53**	Swift; long paths.	July-Aug. .	339	−27	Slow; long.
3 ...	156	+41	Swift.	July-Oct. 8	30	+36	Swift; streaks.
11 ...	220	+13	Swift; streaks.	Aug. 4-10	350	+49	Swift.
17 ...	295	+53	Slow; bright.	10-12	**45**	**+57**	Swift; streaks.
17-25	143	+48	Swift.	10-Sept. 16	353	−11	Rather slow.
25 ...	131	+32	Swift.	Aug. 15 ...	290	+53	Swift; bright.
29 ...	213	+52	Very swift.	21-25	291	+60	Slow; bright.
Feb. 5-10	75	+41	Slow; bright.	25 ...	5	+11	Slow; short.
15 ...	236	+11	Swift; streaks.	Aug.-Sept. .	346	± 0	Slow.
15 ...	261	+ 4	Swift; streaks.	Aug.-Oct. 2	74	+42	Swift; streaks.
20 ...	181	+34	Swift; bright.	Sept. 3-8 .	353	+39	Very swift.
20 ...	263	+36	Swift; streaks.	5-15	62	+37	Swift; streaks.
Mar. 1-4...	166	+ 4	Slow; bright.	6-17	106	+52	Swift; streaks.
14 ...	250	+54	Swift.	15 ...	77	+57	Swift; streaks.
18 ..	316	+76	Slow; bright.	21 ...	31	+19	Slow; trains.
24 ...	161	+58	Swift.	27 ...	75	+15	Swift; streaks.
27 ...	229	+32	Swift; small.	Sep. 29-Oct. 5	189	+73	Swift.
Mar.-Ap.-May	263	+62	Rather swift.	Oct. 2 ...	230	+52	Slow; bright.
Apr. 12-24	210	−10	Slow; fireballs.	4 ...	310	+79	Slowish.
Ap. 17-May 1	240	+47	Small; short.	8 ...	77	+31	Swift; streaks.
Apr. 18-23	189	−31	Slow; long.	8-14	45	+58	Small; short.
20-21	261	+36	Swift; bl. white.	14 ...	133	+68	Rather swift.
20-22	**271**	**+33**	Swift.	16-23	89	+ 8	Swift; streaks.
20-25	218	−31	Slow; long paths.	18-20	**92**	**+15**	Swift; streaks.
30 ...	291	+59	Rather slow.	23 ...	99	+13	Swift; streaks.
Apr.-May .	193	+58	Slow; yellow.	29 ...	109	+23	Very swift.
May 1-6 .	**338**	**− 2**	Swift; streaks.	Nov. 1 ...	43	+22	Slow; bright.
11-18	231	+27	Slow; small.	2 ...	55	+ 9	Slow; bright.
25-June 4	280	+31	Rapid.	10-12	133	+31	Very swift; streaks.
30-Aug.	333	+27	Swift; streaks.	14-16	**150**	**+22**	Swift; streaks.
May-July .	252	−21	Slow; trains.	16-28	154	+41	Swift; streaks.
June 10 ...	261	+ 5	Very slow.	20-23	63	+23	Slow; bright.
11-19	274	+69	Rather swift.	17-23	**25**	**+43**	Very slow; trains.
13 ...	310	+61	Swift; streaks.	25-Dec. 12	316	+60	Slowish.
June-Sep...	335	+57	Swift; slow in Sep.	Nov. 30 ...	190	+58	Swift; streaks.
June-Aug. .	303	+24	Swift.	Dec. 4 ...	162	+58	Swift; streaks.
July 13-20	317	+31	Swift.	6 ...	80	+23	Slow; bright.
15-31	23	+43	Swift; streaks.	8 ...	145	+ 7	Swift; streaks.
19 ...	315	+48	Swift; short.	8 ...	208	+71	Rather swift.
July-Aug. .	269	+48	Slow; trains.	10-12	**108**	**+33**	Swift; short.
15-30	305	−10	Slow; bright.	12 ...	119	+29	Rather swift.
25-Sep. 15	48	+43	Swift; streaks.	22 ...	194	+67	Swift; streaks.
July 25-31	**339**	**−11**	Slow; long.	25 ...	98	+31	Very slow.
July-Sept. .	335	+73	Swift; short.	31 ...	92	+57	Slow; bright.

The radiant-points of the more brilliant showers are indicated by heavier type.

The Perseids, with max. on August 11, are visible for a considerable period and their radiant-point exhibits a motion to E.N.E. amongst the stars. The following is an ephemeris :—

Date.	Radiant.		Date.	Radiant.		Date.	Radiant.	
	α °	δ °		α °	δ °		α °	δ °
July 19 ...	18·9	+50·5	July 29 ...	29·3	+53·8	Aug. 8 ...	41·5	+56·5
21 ...	20·8	+51·1	31 ...	31·6	+54·4	10 ...	44·3	+56·9
23 ...	22·8	+51·8	Aug. 2 ...	33·9	+55·0	12 ...	47·1	+57·3
25 ...	24·9	+52·5	4 ...	36·4	+55·5	14 ...	50·0	+57·7
27 ...	27·1	+53·2	6 ...	38·9	+56·0	16 ...	52·9	+58·0

	Mercury (☿).						Venus (♀).						Mars (♂).					
1907.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1...	17 25 1	22 50 S.	5·4	18 50	22 47	2 44	16 3 27	16 21 S.	43·2	16 45	21 22	1 59	14 28 29	13 33 S.	5·0	14 54	19 47	0 40
11...	18 29 22	24 6	5·0	19 25	23 13	3 1	16 26 17	17 2	36·8	16 33	21 6	1 39	14 52 14	15 26	5·2	14 49	19 32	0 15
21...	19 37 39	23 16	4·8	19 48	23 42	3 36	16 57 29	18 11	31·8	16 32	20 58	1 24	15 16 10	17 9	5·6	14 43	19 16	23 49
31...	20 47 41	20 2	4·8	19 54	0 9	4 24	17 34 42	19 17	27·8	16 36	20 56	1 16	15 40 13	18 41	5·8	14 37	19 1	23 25
Feb. 10...	21 57 50	14 19	5·0	19 50	0 40	5 30	18 16 17	20 0	24·6	16 44	20 59	1 14	16 4 18	20 0	6·2	14 30	18 45	23 0
20...	23 4 25	6 29 S.	5·6	19 36	1 7	6 38	19 0 47	20 2	22·2	16 49	21 4	1 19	16 28 17	21 7	6·8	14 22	18 30	22 38
Mar. 2...	23 53 0	1 8 N.	7·4	19 6	1 16	7 26	19 46 58	19 17	20·2	16 51	21 11	1 31	16 52 1	22 0	7·2	14 12	18 14	22 16
12...	23 58 5	3 38 N.	9·8	18 21	0 42	7 3	20 33 48	17 39	18·4	16 48	21 18	1 48	17 15 18	22 42	7·8	14 0	17 58	21 56
22...	23 29 23	0 22 S.	11·0	17 28	23 28	5 28	21 20 25	15 12	17·0	16 42	21 26	2 10	17 37 52	23 12	8·6	13 46	17 41	21 36
Apr. 1...	23 18 7	4 17	9·8	17 0	22 40	4 20	22 6 21	12 2	15·8	16 30	21 32	2 34	17 59 26	23 32	9·4	13 31	17 23	21 15
11...	23 37 59	4 18	8·2	16 42	22 23	4 4	22 51 28	8 15	15·0	16 16	21 38	3 0	18 19 41	23 44	10·2	13 14	17 4	20 54
21..	0 17 38	1 1 S.	7·0	16 26	22 25	4 24	23 35 53	4 3 S.	14·0	16 0	21 43	3 26	18 38 9	23 52	11·4	12 54	16 43	20 32
May 1...	1 9 57	4 35 N.	6·0	16 11	22 39	5 7	0 19 57	0 24 N.	13·4	15 41	21 47	3 53	18 54 22	23 58	12·6	12 31	16 20	20 9
11...	2 14 22	11 42	5·4	16 0	23 5	6 10	1 4 9	4 55	12·8	15 23	21 52	4 21	19 7 45	24 8	14·0	12 5	15 53	19 41
21...	3 33 49	19 8	5·0	15 59	23 47	7 34	1 49 0	9 20	12·2	15 6	21 58	4 50	19 17 31	24 25	15·6	11 38	15 24	19 10
31...	5 5 8	24 21	5·2	16 9	0 34	8 59	2 35 2	13 26	11·6	14 50	22 4	5 18	19 22 57	24 54	17·4	11 7	14 49	18 31
June 10...	6 30 42	25 20	6·0	16 47	1 20	9 53	3 22 41	17 4	11·2	14 39	22 13	5 47	19 23 16	25 37	19·2	10 33	14 10	17 47
20...	7 37 2	23 2	7·0	17 34	1 47	10 0	4 12 9	20 0	11·0	14 30	22 23	6 16	19 18 7	26 31	21·0	9 56	13 25	16 54
30...	8 19 57	19 19	8·6	18 1	1 50	9 39	5 3 21	22 4	10·6	14 29	22 35	6 41	19 8 13	27 28	22·4	9 15	12 36	15 57
July 10...	8 35 42	15 56	10·4	17 59	1 26	8 53	5 55 52	23 7	10·4	14 34	22 48	7 2	18 55 31	28 16	23·0	8 29	11 44	14 59
20..	8 21 33	14 41	11·4	17 14	0 33	7 52	6 48 59	23 4	10·2	14 48	23 2	7 16	18 43 16	28 46	22·8	7 42	10 52	14 2
30...	7 55 24	16 11	10·6	15 55	23 22	6 49	7 41 46	21 53	10·0	15 10	23 15	7 20	18 34 42	28 54	21·8	6 56	10 5	13 14
Aug. 9...	7 57 20	18 28	8·2	15 6	22 49	6 32	8 33 31	19 39	10·0	15 36	23 27	7 18	18 31 44	28 45	20·6	6 13	9 23	12 33
19...	8 43 46	18 30	6·2	15 16	23 0	6 44	9 23 41	16 29	9·8	16 7	23 38	7 9	18 35 2	28 23	19·0	5 34	8 47	12 0
29...	9 57 14	14 15	5·2	16 15	23 35	6 55	10 12 9	12 32	9·8	16 38	23 47	6 56	18 44 6	27 52	17·4	4 59	8 17	11 35
Sept. 8...	11 10 35	7 3 N.	4·8	17 25	0 5	6 45	10 59 8	8 1	9·8	17 10	23 54	6 38	18 57 59	27 10	16·0	4 27	7 52	11 17
18...	12 15 12	0 48 S.	4·8	18 30	0 30	6 30	11 45 6	3 8 N.	9·8	17 40	0 0	6 20	19 15 46	26 17	14·6	3 58	7 30	11 2
28...	13 13 18	8 11	5·0	19 27	0 49	6 11	12 30 39	1 56 S.	9·8	18 12	0 6	6 0	19 36 26	25 10	13·4	3 31	7 11	10 51
Oct. 8...	14 7 29	14 35	5·4	20 17	1 4	5 51	13 16 29	6 57	9·8	18 45	0 13	5 41	19 59 12	23 48	12·4	3 5	6 55	10 45
18...	14 57 39	19 34	6·2	20 56	1 14	5 32	14 3 16	11 44	10·0	19 17	0 20	5 23	20 23 25	22 10	11·4	2 39	6 40	10 41
28..	15 37 14	22 29	7·2	21 15	1 14	5 13	14 51 37	16 3	10·0	19 49	0 29	5 9	20 48 33	20 17	10·6	2 12	6 25	10 38
Nov. 7...	15 43 18	21 43	9·2	20 38	0 41	4 44	15 41 55	19 42	10·2	20 23	0 40	4 57	21 14 10	18 8	9·8	1 45	6 12	10 39
17...	15 0 21	15 51	9·6	18 32	23 11	3 50	16 34 14	22 26	10·4	20 54	0 53	4 52	21 40 3	15 45	9·2	1 17	5 58	10 39
27...	14 52 1	13 50	7·4	17 38	22 30	3 22	17 28 12	24 5	10·6	21 19	1 7	4 55	22 5 56	13 11	8·6	0 50	5 45	10 40
Dec. 7...	15 32 32	17 16	5·8	18 1	22 33	3 5	18 23 0	24 32	10·8	21 38	1 23	5 8	22 31 45	10 27	8·0	0 21	5 31	10 41
17...	16 30 24	21 16	5·2	18 46	22 53	3 0	19 17 36	23 44	11·0	21 47	1 38	5 29	22 57 27	7 35	7·4	23 52	5 17	10 42
27...	17 35 10	23 56 S.	4·8	19 30	23 19	3 8	20 10 55	21 44 S.	11·4	21 48	1 52	5 56	23 23 1	4 39 S.	7·0	23 23	5 3	10 43

Mercury.

	d h	d h	d h
Gr. elong. E.	Mar. 1 14;	June 26 23;	Oct. 22 22
„ W.	Apr. 14 16;	Aug. 12 15;	Dec. 1 2
Inf. conj. ...	Mar. 17 20:	July 24 15:	Nov. 14 0
Sup. conj. ...	Feb. 2 5;	May 23 21;	Sept. 6 16
Stationary...	Mar. 7 22:	Mar. 30 6;	July 10 4
	Aug. 3 17;	Nov. 3 10;	Nov. 23 2

Venus.

	d h
Gr. elong. W....	Feb. 8 16
Sup. conj.	Sept. 14 13
Gr. brilliancy...	Jan. 4 6

Superior Planets.

	Conjunction.	Opposition.	Quadrature.		Stationary.	
	d h	d h	d h	d h	d h	d h
Mars......	......	July 6 3	Mar. 9 17;	Nov. 11 9	June 5 14;	Aug. 8 18
Jupiter...	July 15 19	......	Mar. 23 9:	Nov. 5 17	Feb. 25 9;	Nov. 30 21
Saturn...	Mar. 8 21	Sept. 17 14	June 19 2;	Dec. 13 16	July 10 14:	Nov. 25 12
Uranus...	......	July 3 8	Apr. 3 1:	Oct. 2 17	Apr. 17 11;	Sept. 18 11
Neptune .	July 5 2	Jan. 2 7	Mar. 31 6;	Oct. 8 20	Mar. 21 13;	Oct. 18 17

	Jupiter (♃).						**Saturn (♄).**				
1907.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.	Sets.	R.A. Noon.	Dec. Noon.	Diam.	Rises.	Transits.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m
Jan. 1	6 23 53	23 15 N.	43·7	3 28	11 42	19 56	22 50 31	9 25 S.	15·2	22 55	4 10
21	6 13 5	23 22	42·6	1 57	10 12	18 27	22 57 19	8 42	14·8	21 40	2 58
Feb. 10	6 6 5	23 27	40·6	0 31	8 47	17 3	23 5 28	7 50	14·6	20 25	1 48
Mar. 2	6 4 30	23 30	38·4	23 11	7 27	15 43	23 14 23	6 54	14·4	19 10	0 38
22	6 8 35	23 31	36·0	21 56	6 12	14 28	23 23 30	5 57	14·4	17 52	23 25
Apr. 11	6 17 37	23 30	33·8	20 47	5 3	13 19	23 32 15	5 3	14·6	16 38	22 15
May 1	6 30 33	23 24	32·0	19 42	3 57	12 13	23 40 8	4 16	15·0	15 23	21 4
21	6 46 20	23 12	30·8	18 40	2 54	11 8	23 46 40	3 38	15·4	14 8	19 52
June 10	7 4 0	22 50	29·8	17 42	1 53	10 5	23 51 21	3 13	15·8	12 51	18 38
30	7 22 44	22 19	29·4	16 45	0 53	9 1	23 53 51	3 2	16·4	11 33	17 21
July 20	7 41 47	21 39	29·2	15 47	23 50	7 53	23 53 56	3 8	17·0	10 16	16 3
Aug. 9	8 0 29	20 52	29·4	14 52	22 50	6 48	23 51 37	3 28	17·4	8 57	14 42
29	8 18 9	20 0	30·0	13 57	21 49	5 41	23 47 19	4 0	17·8	7 37	13 19
Sept. 18	8 34 7	19 8	31·0	12 59	20 46	4 33	23 41 51	4 37	18·0	6 16	11 55
Oct. 8	8 47 37	18 20	32·4	12 0	19 41	3 22	23 36 22	5 12	17·8	4 55	10 31
28	8 57 48	17 42	34·2	10 55	18 32	2 9	23 31 59	5 38	17·4	3 34	9 8
Nov. 17	9 3 46	17 21	36·4	9 44	17 19	0 54	23 29 40	5 50	17·0	2 14	7 47
Dec. 7	9 4 47	17 21	38·6	8 27	16 2	23 37	23 29 52	5 44	16·4	0 55	6 28
27	9 0 36	17 44 N.	40·6	7 2	14 39	22 16	23 32 39	5 22 S.	15·8	23 38	5 13

	Uranus (♅).						**Neptune (♆).**				
Jan. 1	18 37 55	23 28 S.	3·4	20 6	23 58	3 50	6 48 46	22 4 N.	2·7	4 2	12 7
31	18 45 27	23 21	3·4	18 11	22 4	1 57	6 45 18	22 9	2·6	1 59	10 5
Mar. 2	18 51 29	23 15	3·5	16 19	20 12	0 5	6 43 0	22 12	2·6	23 58	8 5
Apr. 1	18 54 54	23 11	3·5	14 23	18 17	22 11	6 42 41	22 13	2·6	22 0	6 7
May 1	18 55 4	23 12	3·6	12 25	16 19	20 13	6 44 29	22 13	2·5	20 4	4 11
31	18 52 12	23 16	3·6	10 25	14 18	18 11	6 48 1	22 9	2·5	18 10	2 16
June 30	18 47 30	23 23	3·8	8 24	12 16	16 8	6 52 35	22 5	2·5	16 16	0 22
July 30	18 42 20	23 28	3·6	6 21	10 13	14 5	6 57 18	21 59	2·5	14 21	22 26
Aug. 29	18 38 52	23 31	3·6	4 20	8 11	12 2	7 1 16	21 53	2·5	12 27	20 31
Sept. 28	18 38 17	23 32	3·6	2 22	6 13	10 4	7 3 43	21 49	2·6	10 32	18 36
Oct. 28	18 40 59	23 29	3·4	0 26	4 17	8 8	7 4 7	21 47	2·6	8 34	16 38
Nov. 27	18 46 30	23 23	3·4	22 34	2 26	6 18	7 2 27	21 49	2·6	6 35	14 39
Dec. 27	18 53 46	23 14 S.	3·4	20 41	0 34	4 27	6 59 17	21 54 N.	2·7	4 34	12 38

Ceres.

Date.	R.A. Noon.	Dec. Noon.	Transits.
1906.	h m	° ′	h m
Dec. 28 ...	4 28	21 5 N.	10 2
1907.			
Jan. 5 ...	4 23	21 24	9 25
13 ...	4 19	21 45	8 50
21 ...	4 17	22 8	8 17
29 ...	4 17	22 33 N.	7 46

Juno.

1907.	R.A. Noon.	Dec. Noon.	Transits.
	h m	° ′	h m
Mar. 26 ...	15 3	4 55 S.	14 50
Apr. 11 ...	14 55	3 7	13 38
27 ...	14 43	1 22 S.	12 23
May 13 ...	14 31	0 2 N.	11 8
29 ...	14 20	0 50	9 55

The times of rising and setting correspond to the adjacent transit, irrespective of the day give the first column.

ECLIPSES IN 1907.

In the year 1907 there will be two Eclipses of the Sun and two of the Moon.

I.—*A Total Eclipse of the Sun, January* 13.

Invisible at Greenwich. The centre of the shadow begins its course in Russia, crosses the north part of the Caspian Sea, runs through Turkestan and Mongolia, and the eclipse ends in Eastern Siberia. The following details are for Ura-tiube, an easily accessible place in Turkestan, 2 miles south of the central line, Latitude 39° 59′ N., longitude 69° 0′ E. Eclipse begins $20^{h}\ 50^{m}\ 52^{s}$. Totality begins $22^{h}\ 6^{m}\ 37^{s}\cdot1$. Totality ends $22^{h}\ 8^{m}\ 40^{s}\cdot9$. Eclipse ends $23^{h}\ 31^{m}\ 1^{s}$. Duration of totality $2^{m}\ 3^{s}\cdot8$. Sun's altitude 22° 21′.

A Partial Eclipse will be visible at Madras and at Hongkong.

	Madras.				Hongkong.			
		d	h	m		d	h	m
Eclipse begins	Jan.	13	21	47·1	Jan.	14	1	29·1
Greatest phase	,,	13	23	2·4	,,	14	2	41·2
Eclipse ends	,,	14	0	23·4	,,	14	3	45·3
Magnitude of eclipse............		0·462				0·467		

The times are local mean in all cases.

II.—*A Partial Eclipse of the Moon, January* 28–29.

Invisible at Greenwich. The eclipse will be seen from the greater part of North America, Australia, and almost the whole of Asia.

	d	h	m		d	h	m
First contact with Penumbra	28	22	45·7	With shadow	29	0	6·0
Last ,, ,, ...	29	4	30·3	,, ,,	29	3	10·0

Magnitude of the Eclipse (Moon's diameter = 1) 0·715.

III.—*An Annular Eclipse of the Sun, July* 10.

Invisible at Greenwich. The line of Central Eclipse crosses South America in a curved line (convexity northwards) which cuts the West and East coasts respectively nearly in latitude 20° and 18° south.

Begins on the Earth generally July $10^{d}\ 0^{h}\ 34^{m}\cdot9$ G.M.T. in longitude 88° 39′ W., latitude 20° 37′ S. Ends on the Earth generally July $10^{d}\ 6^{h}\ 14^{m}\cdot3$ in longitude 12° 33′ W., latitude 23° 31′ S.

IV.—*A Partial Eclipse of the Moon, July* 24.

Partly visible at Greenwich. Magnitude of the Eclipse (Moon's diameter = 1) 0·620. The first contact with the shadow occurs at 41° from the N. point towards the East.

	d	h	m		h	m
First contact with Penumbra	24	13	58·7	With shadow	15	3·7
Last ,, ,, ...	24	18	46·1	,, ,,	17	41·1
Middle of the Eclipse	24	16	22			

The Moon will set at Greenwich at $16^{h}\ 10^{m}$ partly eclipsed.

TRANSIT OF MERCURY.

There will be a Transit of Mercury on Nov. 14 (civil day), being visible at Greenwich about Noon.

Ingress.		d	h	m	s	Egress.	d	h	m	s
External Contact ...	Nov.	13	22	23	40	External Contact ...	14	1	49	58
Internal ,, ...	,,	13	22	26	18	Internal ,, ...	14	1	47	17

Least distance of Centres 12′ 38″·4 at $14^{d}\ 0^{h}\ 6^{m}\ 51^{s}$.

Angle from North Point of Contact at Ingress 62° towards the East, at Egress 345° towards East.

OCCULTATIONS, 1907. (VISIBLE AT GREENWICH.)

e.	Star.	Mag.	Disap.		Reap.	
			M. T.	P.	M. T.	P.
l.			h m	°	h m	°
	B.A.C. 3837 ...	5·9	17 8	118	18 23	295
	B.D.−9° 3975 .	7·0	……	…	18 18	0
	γ Libræ	4·1	17 44	54	18 20	352
	39 Aquarii	6·5	5 27	39	6 24	276
	ξ^2 Ceti............	4·3	9 7	94	10 12	224
	B.D.+11° 445 .	6·8	5 55	62	……	…
	B.D.+12° 473 .	6·2	14 28†	149	14 46†	187
	B.D.+19° 1110	6·1	15 26	56	16 14	305
	χ^2 Orionis	5·9	16 32	84	17 25‡	277
	ν Geminorum...	4·1	6 40	112	7 45	231
	56 Geminorum .	5·0	7 39	38	8 27	317
	61 Geminorum .	5·7	10 30	87	11 52	282
	θ Cancri	5·6	18 9	147	18 49	239
	l Leonis	5·3	9 19	61	10 5	333
'.						
	B.D.−8° 3737 .	6·0	13 37	142	14 36	270
	B.D.−8° 3740 .	6·5	14 5	159	14 54	253
	14 Sagittarii ...	5·9	16 41†	138	17 30	243
	B.D.+1° 241...	6·9	5 36	87	……	…
	B.D.+16° 625 .	7·0	9 4	98	……	…
	ν Geminorum .	4·1	16 8‡	83	16 58†	282
	ζ Geminorum .	Var	7 11	70	8 29	288
	85 Geminorum .	5·3	7 7	50	8 5	316
	δ Cancri	4·2	5 30	69	6 31	302
	B.D.+17° 1979	6·8	13 34	165	……	…
r.						
	ξ^1 Libræ	5·9	11 50	90	12 48	320
	20 Capricorni...	6·2	16 52†	123	17 40	224
	63 Tauri	5·6	12 2	68	12 53†	278
	m Tauri	5·1	9 24	123	10 17	228
	χ^1 Orionis	4·7	7 16	64	8 30	290
	χ^4 Orionis	4·8	12 30	95	13 25	268
	B.D.+20° 1856	6·8	7 18	123	……	…
	B.A.C. 3837 ...	5·9	14 53	76	15 42	331
	b Virginis	5·2	12 13	159	13 9	258
	80 Virginis	5·8	7 42	180	8 4	227
	n Virginis	7·0	14 44	138	15 49	274
	B.D.−5° 3762 .	6·8	……	…	15 18	280
r.						
	58 Ophiuchi ...	5·0	17 34	144	18 24	229
	γ Capricorni ...	3·8	16 18	29	17 1	308
	B.D.+20° 1427	6·9	7 23	106	……	…
	56 Geminorum .	5·0	9 24	150	10 6	228
	61 Geminorum .	5·7	11 53	147	12 30	230
	B.D.−13° 411 .	7·0	……	…	13 59	273
	ξ Ophiuchi......	4·5	15 38	104	16 50	273
y						
	B.A.C. 6607 ...	5·7	12 38*	131	13 29	235
	B.D.−21° 5729.	6·5	16 24	150	16 51	190
	n Virginis	7·0	10 19	180	10 54	237
	B.D.−5° 3762 .	6·8	9 37	170	……	…
	B.D.−11° 3814	7·0	14 4	37	……	…
	θ Libræ	4·3	16 6	64	16 52†	321
	58 Ophiuchi ..	5·0	8 16†	117	9 12*	269

Date.	Star.	Mag.	Disap.		Reap.
			M. T.	P.	M. T.
May			h m	°	h m
29	ν^1 Sagittarii ...	5·0	10 46	129	11 41
29	ν^2 Sagittarii ...	5·2	11 5	107	12 11
29	B.D.−22° 4928	7·0	……	…	13 44
29	B.A.C. 6485 ...	6·5	14 13	97	15 25
31	B.D.−19° 5998	2·0	13 2	299	……
June					
13	79 Geminorum .	6·5	9 55	121	10 42†
21	B.D.−8° 3737 .	6·0	8 4	71	8 55
21	B.D.−8° 3740 .	6·5	8 24	87	9 28
22	B.D.−13° 4111	7·0	9 40	153	……
24	ξ Ophiuchi	4·5	11 40	82	12 48
24	B.A.C. 5866 ...	6·5	13 36	126	14 34
27	B.D.−11° 5768	6·6	……	…	12 10
July					
6	63 Tauri	5·6	13 35†	73	14 28
14	*l* Leonis	5·3	10 0	125	10 51†
20	θ Libræ	4·3	11 58	85	12 57†
23	ν^1 Sagittarii ...	5·0	7 57	108	9 3
23	ν^2 Sagittarii ...	5·2	8 21	89	9 30
23	B.D.−22° 4928	7·0	10 6	48	……
23	B.A.C. 6485 ...	6·5	11 32	87	12 43
23	Lalande 35530 .	7·0	13 3	149	……
23	B.D.−6° 6335 .	7·0	……	…	10 49
28	30 Piscium......	4·7	11 10	79	12 14
28	33 Piscium......	4·6	13 4	93	14 6
29	20 Ceti	4·9	11 23	126	11 55
31	ξ^2 Ceti............	4·3	10 40†	25	11 18
Aug.					
4	χ^1 Orionis	4·7	15 38	127	16 20
18	B.A.C. 5866 ...	6·5	7 52	122	9 1
21	B.D.−21° 5768	6·8	8 21	54	……
22	B.D.−17° 6389	6·7	14 37	121	……
23	74 Aquarii	5·8	17 11	44	18 4†
24	B.D.−8° 6166 .	7·0	……	…	13 28
Sept.					
12	B.D.−13° 4111	7·0	7 21	93	……
13	B.D.−17° 4502	6·5	8 14	178	8 30
20	ψ^3 Aquarii	5·2	13 34	68	14 40
21	30 Piscium......	4·7	7 10	106	7 59
21	33 Piscium......	4·6	9 4	125	9 37
24	μ Ceti	4·4	12 51	106	13 46
26	δ^2 Tauri	4·7	11 57	44	12 59
27	B.A.C. 1639 ...	6·5	14 31	27	15 24
27	B.A.C. 1651 ...	6·5	15 29	140	16 5
Oct.					
2	8 Leonis	5·9	16 4	86	17 11
13	B.A.C. 6448 ...	5·9	8 14	108	9 15‡
14	B.A.C. 6850 ...	6·5	9 19	100	10 19‡
16	B.D.−17° 6389	6·7	5 34	12	……
17	B.D.−12° 6374	7·0	9 53	99	……
18	B.D.−8° 6166 .	7·0	5 55	61	……
21	B.D.+7° 362...	7·0	……	…	10 13
23	B.D.+16° 569 .	6·4	17 12	36	18 6

† Star below horizon. ‡ Star setting. * Star rising.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from the *bottom of the Moon's inverted* image towards the right.

Date.	Star.	Mag.	Disap. M.T.	Disap. P.	Reap. M.T.	Reap. P.
Oct.			h m	°	h m	°
24	B.A.C. 1468 ...	6·5	7 15	21	7 47	307
24	*i* Tauri	5·1	9 16	81	10 18	244
24	B.A.C. 1563 ...	6·5	17 52	68	19 1	279
27	63 Geminorum .	5·3	10 10	81	11 7	272
28	B.D. +20° 2079	7·0	......	...	14 24	272
29	83 Cancri	6·6	15 46	103	17 6	283
Nov.						
16	B.D. +1° 241...	6·9	12 5	84	......	...
18	μ Ceti	4·4	5 7	87	6 1	226
20	δ¹ Tauri	3·9	4 25†	36	5 4	292
20	δ² Tauri	4·7	4 44†	77	5 35	250
20	B.D. +17° 750 (mean).........	6·5	9 25	119	10 12	200
20	B.A.C. 1468 ...	6·5	17 31	154	17 51	192
21	B.A.C. 1639 ...	6·5	6 27	65	7 21	267
21	B.D. +20° 969 .	7·0	......	...	13 14	203
21	ζ Tauri	3·0	17 6	42	17 54	313
22	B.D. +21° 1163	6·7	......	...	8 45	316
23	δ Geminorum .	3·6	15 41	81	17 1	291
24	B.D. +21° 1792	7·0	......	...	18 56	288
Dec.						
8	4 Capricorni ...	6·0	5 15	22	5 54	310
11	B.D. −11° 6032	6·3	6 7	75	7 18	228

Date.	Star.	Mag.	Disap. M.T.	Disap. P.	M.
Dec.			h m	°	h
12	30 Piscium......	4·7	3 35	23	4
12	33 Piscium......	4·6	5 28	51	6
12	B.A.C. 17	5·9	8 41	104	9
13	20 Ceti	4·9	3 57	110	4
15	ξ² Ceti	4·3	2 55	93	3
17	δ¹ Tauri	3·9	12 53	147	13
17	δ³ Tauri	4·2	14 9	77	15
17	B.D. +17° 750 (mean)	6·5	18 13	127	18
18	B.A.C. 1563 ...	6·5	7 15	50	8
18	107 Tauri	6·5	9 1	93	10
18	B.A.C. 1639 ...	6·5	15 52	155	16
19	B.D. +21° 1072	6·9	......	...	9
19	B.D. +21° 1146	6·6	......	...	16
19	B.D. +21° 1163	6·7	......	...	18
22	B.D. +20° 2150	6·6	......	...	13
22	B.D. +20° 2158	6·9	......	...	13
22	B.D. +20° 2159	7·0	......	...	13
28	80 Virginis......	5·8	19 47	171	20
29	B.D. −8° 3737 .	6·0	14 2†	105	15
29	B.D. −8° 3740 .	6·5	14 22*	120	15
30	B.D. −13° 4111	7·0	......	...	17
31	B.D. −17° 4502	6·5	16 34†	176	16
31	B.A.C. 5408 ...	6·5	18 21	116	19

† Star below horizon. * Star rising.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i. e.* from t bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are es interesting to observers rather South and rather North of Greenwich respectively

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	An
			h m	°				h m	
Jan. 1	δ Cancri	4·2	17 50	16	July 4	μ Ceti	4·4	12 54	34
7	B.A.C. 4572 ...	6·4	16 54	28	Aug. 17	χ Ophiuchi......	5·0	9 23	1
Mar. 3	B.D. −6° 3911 .	6·5	10 57	25	Sept. 14	B.A.C. 5746 ...	6·5	6 31	1
5	49 Libræ.........	5·6	18 5	18	26	δ¹ Tauri	3·9	11 57	34
24	δ Cancri	4·2	16 24	13	Nov. 20	δ³ Tauri	4·2	5 58	34
Apr. 23	*l* Leonis	5·3	12 28	24	Dec. 17	B.D. +16° 569 .	6·4	6 52	33
27	B.D. − 8° 3740 .	6·5	12 52	26	22	η Cancri	5·5	8 15	

South Near Approaches.

			h m	°				h m	
Jan. 1	*d*¹ Cancri	6·0	5 58	182	May 17	85 Geminorum .	5·3	9 7	19
5	*b* Virginis	5·2	15 52	207	21	B.A.C. 3837 ...	5·9	9 22	20
7	*n* Virginis	7·0	20 32	206	July 28	B.A.C. 17	5·9	16 33	15
Feb. 25	B.A.C. 3029 ...	6·5	12 52	197	Nov. 20	*i* Tauri	5·1	20 3	17
Mar. 5	θ Libræ	4·3	14 52	202	Dec. 20	*d* Geminorum .	5·2	8 46	17
21	B.D. +19° 1110	6·1	6 28	173					

JUPITER'S SATELLITES, 1907.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re.; *t*, Tr. Ing.; T, Tr. Egr.

January.

h	m	
0	50	i. *t*.
3	7	i. T.
21	58	i. *o*.
0	21	i. E.
12	58	ii. *t*.
15	48	ii. T.
19	16	i. *t*.
21	33	i. T.
16	24	i. *o*.
18	50	i. E.
20	6	iii. *o*.
23	45	iii. E.
7	54	ii. *o*.
11	3	ii. E.
13	42	i. *t*.
15	59	i. T.
10	50	i. *o*.
13	18	i. E.
2	6	ii. *t*.
4	56	ii. T.
8	8	i. *t*.
10	25	i. T.
5	16	i. *o*.
7	47	i. E.
9	56	iii. *t*.
12	56	iii. T.
14	43	iv. *o*.
16	7	iv. O.
17	11	iv. *e*.
18	50	iv. E.
21	0	ii. *o*.
0	20	ii. E.
2	34	i. *t*.
4	51	i. T.
23	43	i. *o*.
2	16	i. E.
15	14	ii. *t*.
18	4	ii. T.
21	0	i. *t*.
23	17	i. T.
18	9	i. *o*.
20	45	i. E.
23	23	iii. *o*.
3	46	iii. E.
10	7	ii. *o*.
13	38	ii. E.
15	26	i. *t*.
17	43	i. T.
12	35	i. *o*.
15	13	i. E.
4	22	ii. *t*.
7	12	ii. T.
9	52	i. *t*.
2	9	i. T.

Jan. (con.).

d	h	m	
14	7	1	i. *o*.
	9	42	i. E.
	13	13	iii. *t*.
	16	13	iii. T.
	23	15	ii. *o*.
15	2	55	ii. E.
	4	18	i. *t*.
	6	35	i. T.
	22	15	iv. *t*.
	23	53	iv. T.
16	1	27	i. *o*.
	4	11	i. E.
	17	30	ii. *t*.
	20	20	ii. T.
	22	44	i. *t*.
17	1	1	i. T.
	19	54	i. *o*.
	22	40	i. E.
18	2	42	iii. *o*.
	7	47	iii. E.
	12	22	ii. *o*.
	16	13	ii. E.
	17	10	i. *t*.
	19	27	i. T.
19	14	20	i. *o*.
	17	8	i. E.
20	6	39	ii. *t*.
	9	29	ii. T.
	11	37	i. *t*.
	13	54	i. T.
21	8	46	i. *o*.
	11	37	i. E.
	16	33	iii. *t*.
	19	33	iii. T.
22	1	30	ii. *o*.
	5	30	ii. E.
	6	3	i. *t*.
	8	20	i. T.
23	3	13	i. *o*.
	6	6	i. E.
	19	48	ii. *t*.
	22	38	ii. T.
24	0	29	i. *t*.
	2	46	i. T.
	5	1	iv. *o*.
	6	32	iv. O.
	11	10	iv. *e*.
	13	8	iv. E.
	21	39	i. *o*.
25	0	35	i. E.
	6	3	iii. *o*.
	11	48	iii. E.
	14	39	ii. *o*.
	18	48	ii. E.

Jan. (con.).

d	h	m	
25	18	56	i. *t*.
	21	13	i. T.
26	16	6	i. *o*.
	19	4	i. E.
27	8	58	ii. *t*.
	11	48	ii. T.
	13	22	i. *t*.
	15	39	i. T.
28	10	32	i. *o*.
	13	33	i. E.
	19	57	iii. *t*.
	22	57	iii. T.
29	3	47	ii. *o*.
	7	49	i. *t*.
	8	5	ii. E.
	10	6	i. T.
30	4	59	i. *o*.
	8	1	i. E.
	22	7	ii. *t*.
31	0	57	ii. T.
	2	15	i. *t*.
	4	32	i. T.
	23	26	i. *o*.

February.

d	h	m	
1	2	30	i. E.
	9	28	iii. *o*.
	12	28	iii. O.
	12	51	iii. *e*.
	12	56	iv. *t*.
	14	43	iv. T.
	15	49	iii. E.
	16	57	ii. *o*.
	20	42	i. *t*.
	21	23	ii. E.
	22	59	i. T.
2	17	53	i. *o*.
	20	59	i. E.
3	11	18	ii. *t*.
	14	8	ii. T.
	15	9	i. *t*.
	17	26	i. T.
4	12	20	i. *o*.
	15	28	i. E.
	23	23	iii. *t*.
5	2	24	iii. T.
	6	6	ii. *o*.
	9	35	i. *t*.
	10	41	ii. E.
	11	52	i. T.
6	6	47	i. *o*.
	9	57	i. E.
7	0	29	ii. *t*.

Feb. (con.).

d	h	m	
7	3	19	ii. T.
	4	2	i. *t*.
	6	19	i. T.
8	1	13	i. *o*.
	4	26	i. E.
	12	57	iii. *o*.
	15	58	iii. O.
	16	51	iii. *e*.
	19	17	ii. *o*.
	19	50	iii. E.
	22	29	i. *t*.
	23	58	ii. E.
9	0	46	i. T.
	19	41	i. *o*.
	20	3	iv. *o*.
	21	47	iv. O.
	22	55	i. E.
10	5	11	iv. *e*.
	7	25	iv. E.
	13	41	ii. *t*.
	16	31	ii. T.
	16	56	i. *t*.
	19	13	i. T.
11	14	8	i. *o*.
	17	23	i. E.
12	2	55	iii. *t*.
	5	57	iii. T.
	8	28	ii. *o*.
	11	23	i. *t*.
	13	16	ii. E.
	13	40	i. T.
13	8	35	i. *o*.
	11	52	i. E.
14	2	53	ii. *t*.
	5	43	ii. T.
	5	50	i. *t*.
	8	7	i. T.
15	3	3	i. *o*.
	6	21	i. E.
	16	32	iii. *o*.
	19	33	iii. O.
	20	51	iii. *e*.
	21	39	ii. *o*.
	23	51	iii. E.
16	0	18	i. *t*.
	2	33	ii. E.
	2	35	i. T.
	21	30	i. *o*.
17	0	50	i. E.
	16	6	ii. *t*.
	18	45	i. *t*.
	18	57	ii. T.
	21	2	i. T.
18	4	30	iv. *t*.

Feb. (con.).

d	h	m	
18	6	26	iv. T.
	15	58	i. *o*.
	19	19	i. E.
19	6	31	iii. *t*.
	9	33	iii. T.
	10	51	ii. *o*.
	13	12	i. *t*.
	15	29	i. T.
	15	51	ii. E.
20	10	25	i. *o*.
	13	48	i. E.
21	5	19	ii. *t*.
	7	40	i. *t*.
	8	9	ii. T.
	9	57	i. T.
22	4	53	i. *o*.
	8	17	i. E.
	20	11	iii. *o*.
	23	13	iii. O.
23	0	4	ii. *o*.
	0	52	iii. *e*.
	2	8	i. *t*.
	3	53	iii. E.
	4	25	i. T.
	5	8	ii. E.
	23	21	i. *o*.
24	2	46	i. E.
	18	34	ii. *t*.
	20	35	i. *t*.
	21	25	ii. T.
	22	52	i. T.
25	17	48	i. *o*.
	21	15	i. E.
26	10	12	iii. *t*.
	12	5	iv. *o*.
	13	14	iii. T.
	13	17	ii. *o*.
	13	59	iv. O.
	15	3	i. *t*.
	17	20	i. T.
	18	26	ii. E.
	23	13	iv. *e*.
27	1	40	iv. E.
	12	16	i. *o*.
	15	44	i. E.
28	7	49	ii. *t*.
	9	30	i. *t*.
	10	39	ii. T.
	11	48	i. T.

March.

d	h	m	
1	6	44	i. *o*.
	10	13	i. E.

March (con.).

d	h	m	
1	23	55	iii. *o*.
2	2	31	ii. *o*.
	2	58	iii. O
	3	58	i. *t*.
	4	52	iii. *e*.
	6	16	i. T.
	7	44	ii. E.
	7	54	iii. E
3	1	12	i. *o*.
	4	41	i. E.
	21	5	ii. *t*.
	22	26	i. *t*.
	23	56	ii. T.
4	0	43	i. T.
	19	40	i. *o*.
	23	10	i. E.
5	13	58	iii. *t*.
	15	46	ii. *o*.
	16	54	i. *t*.
	17	1	iii. T
	19	11	i. T
	21	1	ii. E.
6	14	9	i. *o*.
	17	39	i. E.
	21	5	iv. *t*.
	23	9	iv. T
7	10	20	ii. *t*.
	11	22	i. *t*.
	13	11	ii. T.
	13	40	i. T.
8	8	37	i. *o*.
	12	8	i. E.
9	3	44	iii. *o*
	5	1	ii. *o*.
	5	50	i. *t*.
	6	48	iii.
	8	8	i. T.
	8	53	iii. *e*
	10	19	ii. E
	11	56	iii.
10	3	5	i. *o*.
	6	37	i. E.
	23	38	ii. *t*.
11	0	19	i. *t*.
	2	29	ii. T
	2	36	i. T.
	21	34	i. *o*.
12	1	6	i. E
	17	49	iii. *t*
	18	16	ii. *o*.
	18	47	i. *t*.
	20	52	iii.
	21	4	i. T.
	23	37	ii. E

March (con.).

d	h	m	
13	16	2	i. *o*.
	19	35	i. E.
14	12	55	ii. *t*.
	13	15	i. *t*.
	15	33	i. T.
	15	46	ii. T.
15	5	8	iv. *o*.
	7	13	iv. O.
	10	31	i. *o*.
	14	4	i. E.
	17	15	iv. *e*.
	19	55	iv. E.
16	7	33	ii. *o*.
	7	38	iii. *o*.
	7	44	i. *t*.
	10	1	i. T.
	10	42	iii. O.
	12	53	iii. *e*.
	12	54	ii. E.
	15	57	iii. E.
17	4	59	i. *o*.
	8	33	i. E.
18	2	12	i. *t*.
	2	14	ii. *t*.
	4	30	i. T.
	5	4	ii. T.
	23	28	i. *o*.
19	3	2	i. E.
	20	41	i. *t*.
	20	49	ii. *o*.
	21	44	iii. *t*.
	22	58	i. T.
20	0	49	iii. T.
	2	12	ii. E.
	17	57	i. *o*.
	21	31	i. E.
21	15	9	i. *t*.
	15	32	ii. *t*.
	17	26	i. T.
	18	22	ii. T.
22	12	26	i. *o*.
	16	0	i. E.
23	9	38	i. *t*.
	10	6	ii. *o*.
	11	36	iii. *o*.
	11	55	i. T.
	14	36	iv. *t*.
	14	41	iii. O.
	15	30	ii. E.
	16	52	iv. T.
	16	53	iii. *e*.
	19	58	iii. E.
24	6	54	i. *o*.
	10	28	i. E.
25	4	7	i. *t*.
	4	51	ii. *t*.
	6	24	i. T.
	7	42	ii. T.
26	1	23	i. *o*.
	4	57	i. E.
	22	36	i. *t*.
27	23	24	ii. *o*.
	0	53	i. T.
	1	*43*	*iii. t.*

March (con.).

d	h	m	
27	4	47	ii. E.
	4	51	iii. T.
	19	52	i. *o*.
	23	26	i. E.
28	17	4	i. *t*.
	18	10	ii. *t*.
	19	22	i. T.
	21	1	ii. T.
29	14	21	i. *o*.
	17	55	i. E.
30	11	33	i. *t*.
	12	42	ii. *o*.
	13	51	i. T.
	15	37	iii. *o*.
	18	5	ii. E.
	18	43	iii. O.
	20	53	iii. *e*.
	23	59	iii. E.
31	8	50	i. *o*.
	12	24	i. E.
	23	8	iv. *o*.

April.

d	h	m	
1	1	27	iv. O.
	6	2	i. *t*.
	7	29	ii. *t*.
	8	20	i. T.
	10	20	ii. T.
	11	19	iv. *e*.
	14	10	iv. E.
2	3	20	i. *o*.
	6	53	i. E.
3	0	31	i. *t*.
	2	1	ii. *o*.
	2	49	i. T.
	5	48	iii. *t*.
	7	23	ii. E.
	8	55	iii. T.
	21	49	i. *o*.
4	1	22	i. E.
	19	1	i. *t*.
	20	50	ii. *t*.
	21	18	i. T.
	23	41	ii. T.
5	16	18	i. *o*.
	19	51	i. E.
6	13	30	i. *t*.
	15	20	ii. *o*.
	15	47	i. T.
	19	43	iii. *o*.
	20	40	ii. E.
	22	50	iii. O.
7	0	53	iii. *e*.
	4	0	iii. E.
	10	48	i. *o*.
	14	20	i. E.
8	7	59	i. *t*.
	10	12	ii. *t*.
	10	16	i. T.
	13	3	ii. T.
9	5	17	i. *o*.
	8	*49*	*i. E.*
	8	*58*	*iv. t.*

April (con.).

d	h	m	
9	11	27	iv. T.
10	2	28	i. *t*.
	4	40	ii. *o*.
	4	46	i. T.
	9	55	iii. *t*.
	9	58	ii. E.
	13	2	iii. T.
	23	46	i. *o*.
11	3	18	i. E.
	20	58	i. *t*.
	23	15	i. T.
	23	32	ii. *t*.
12	2	24	ii. T.
	18	16	i. *o*.
	21	47	i. E.
13	15	27	i. *t*.
	17	44	i. T.
	18	0	ii. *o*.
	23	16	ii. E.
	23	52	iii. *o*.
14	3	1	iii. O.
	4	54	iii. *e*.
	8	2	iii. E.
	12	45	i. *o*.
	16	15	i. E.
15	9	56	i. *t*.
	12	14	i. T.
	12	54	ii. *t*.
	15	46	ii. T.
16	7	15	i. *o*.
	10	44	i. E.
17	4	26	i. *t*.
	6	43	i. T.
	7	20	ii. *o*.
	12	34	ii. E.
	14	5	iii. *t*.
	17	13	iii. T.
	17	57	iv. *o*.
	20	28	iv. O.
18	1	45	i. *o*.
	5	13	i. E.
	5	22	iv. *e*.
	8	24	iv. E.
	22	55	i. *t*.
19	1	13	i. T.
	2	16	ii. *t*.
	5	7	ii. T.
	20	14	i. *o*.
	23	42	ii. E.
20	17	25	i. *t*.
	19	43	i. T.
	20	40	ii. *o*.
21	1	52	ii. E.
	4	5	iii. *o*.
	7	14	iii. O.
	8	54	iii. *e*.
	12	3	iii. E.
	14	44	i. *o*.
	18	11	i. E.
22	11	54	i. *t*.
	14	12	i. T.
	15	39	ii. *t*.
	18	30	ii. T.
23	9	14	i. *o*.

April (con.).

d	h	m	
23	12	40	i. E.
24	6	24	i. *t*.
	8	42	i. T.
	10	1	ii. *o*.
	15	9	ii. E.
	18	18	iii. *t*.
	21	28	iii. T.
25	3	44	i. *o*.
	7	9	i. E.
26	0	53	i. *t*.
	3	11	i. T.
	4	3	iv. *t*.
	5	1	ii. *t*.
	6	45	iv. T.
	7	53	ii. T.
	22	13	i. *o*.
27	1	38	i. E.
	19	24	i. *t*.
	21	41	i. T.
	23	23	ii. *o*.
28	4	27	ii. E.
	8	20	iii. *o*.
	11	31	iii. O.
	12	54	iii. *e*.
	16	4	iii. E.
	16	43	i. *o*.
	20	6	i. E.
29	13	53	i. *t*.
	16	11	i. T.
	18	24	ii. *t*.
	21	16	ii. T.
30	11	13	i. *o*.
	14	35	i. E.

May.

d	h	m	
1	8	23	i. *t*.
	10	41	i. T.
	12	45	ii. *o*.
	17	45	ii. E.
	22	34	iii. *t*.
2	1	45	iii. T.
	5	43	i. *o*.
	9	4	i. E.
3	2	53	i. *t*.
	5	11	i. T.
	7	47	ii. *t*.
	10	39	ii. T.
4	0	13	i. *o*.
	3	33	i. E.
	13	24	iv. *o*.
	16	10	iv. O.
	21	23	i. *t*.
	23	25	iv. *e*.
	23	41	i. T.
5	2	7	ii. *o*.
	2	37	iv. E.
	7	3	ii. E.
	12	37	iii. *o*.
	15	49	iii. O.
	16	54	iii. *e*.
	18	43	i. *o*.
	20	5	iii. E.
	22	2	i. E.

May (con.).

d	h	m	
6	15	52	i. *t*.
	18	10	i. T.
	21	11	ii. *t*.
7	0	3	ii. T.
	13	13	i. *o*.
	16	31	i. E.
8	10	22	i. *t*.
	12	40	i. T.
	15	29	ii. *o*.
	20	21	ii. E.
9	2	52	iii. *t*.
	6	5	iii. T.
	7	43	i. *o*.
	10	59	i. E.
10	4	52	i. *t*.
	7	10	i. T.
	10	34	ii. *t*.
	13	26	ii. T.
11	2	13	i. *o*.
	5	28	i. E.
	23	22	i. *t*.
12	1	40	i. T.
	4	52	ii. *o*.
	9	39	ii. E.
	16	57	iii. *o*.
	20	10	iii. O.
	20	43	i. *o*.
	20	54	iii. *e*.
	23	43	iv. *t*.
	23	57	i. E.
13	0	5	iii. E.
	2	37	iv. T.
	17	52	i. *t*.
	20	10	i. T.
	23	58	ii. *t*.
14	2	51	ii. T.
	15	13	i. *o*.
	18	26	i. E.
15	12	22	i. *t*.
	14	40	i. T.
	18	15	ii. *o*.
	22	57	ii. E.
16	7	13	iii. *t*.
	9	43	i. *o*.
	10	27	iii. T.
	12	55	i. E.
17	6	52	i. *t*.
	9	10	i. T.
	13	22	ii. *t*.
	16	14	ii. T.
18	4	13	i. *o*.
	7	23	i. E.
19	1	22	i. *t*.
	3	40	i. T.
	7	38	ii. *o*.
	12	15	ii. *e*.
	21	18	iii. *o*.
	22	43	i. *o*.
20	0	32	iii. O.
	0	53	iii. *e*.
	1	52	i. E.
	4	6	iii. E.
	19	52	i. t.
	22	10	i. T.

May (

d	h	m
21	2	46
	5	39
	9	22
	12	22
	17	13
	17	28
	20	21
	20	49
22	14	23
	16	41
	21	1
23	1	33
	11	35
	11	4
	14	5
	14	5
24	8	5
	11	11
	16	1
	19	3
25	6	1
	9	1
26	3	2
	5	4
	10	2
	14	5
27	0	4
	1	4
	3	4
	8	
	21	5
28	0	1
	5	3
	8	2
	19	1
	22	1
29	16	2
	18	4
	19	4
	22	5
	23	48
30	4	9
	13	44
	15	58
	16	45
	19	1
31	10	5
	13	1
	18	5
	21	5

Jun

d	h	m
1	8	1
	11	1
2	5	2
	7	4
	13	1
	17	2
3	2	4
	5	4
	6	
	12	
	2	

June (con.).

d	h	m	
4	2	12	i. T.
	8	24	ii. *t.*
	11	17	ii. T.
	21	15	i. *o.*
5	0	11	i. E.
	18	24	i. *t.*
	20	43	i. T.
5	2	37	ii. *o.*
	6	45	ii. E.
	15	46	i. *o.*
	18	40	i. E.
	20	22	iii. *t.*
	23	41	iii. T.
7	5	38	iv. *o.*
	8	53	iv. O.
	11	31	iv. *e.*
	12	54	i. *t.*
	15	0	iv. E.
	15	13	i. T.
	21	49	ii. *t.*
8	0	42	ii. T.
	10	16	i. *o.*
	13	9	i. E.
9	7	25	i. *t.*
	9	43	i. T.
	16	1	ii. *o.*
	20	3	ii. E.
0	4	46	i. *o.*
	7	37	i. E.
	10	31	iii. *o.*
	16	8	iii. E.
1	1	55	i. *t.*
	4	13	i. T.
	11	14	ii. *t.*
	14	7	ii. T.
	23	16	i. *o.*
2	2	6	i. E.
	20	25	i. *t.*
	22	43	i. T.
3	5	25	ii. *o.*
	9	21	ii. E.
	17	47	i. *o.*
	20	35	i. E.
4	0	48	iii. *t.*
	4	8	iii. T.
	14	56	i. *t.*
	17	14	i. T.
5	0	38	ii. *t.*
	3	32	ii. T.
	12	17	i. *o.*
	15	3	i. E.
	16	4	iv. *t.*
	19	27	iv. T.
6	9	26	i. *t*
	11	44	i. T.
	18	50	ii. *o.*
	22	39	ii. E.

August.

d	h	m	
	0	43	iii. e.
	6	8	iii. O.
		36	ii t.
		31	ii. T.

Aug. (con.).

d	h	m	
14	17	0	i. *e.*
	19	46	i. O.
15	14	37	i. *t.*
	16	57	i. T.
16	5	57	ii. *e.*
	9	53	ii. O.
	11	28	i. *e.*
	14	16	i. O.
17	9	8	i. *t.*
	11	27	i. T.
	16	53	iii. *t.*
	20	26	iii. T.
18	1	59	ii. *t.*
	4	55	ii. T.
	5	57	i. *e.*
	8	46	i. O.
19	3	38	i. *t.*
	5	57	i. T.
	19	15	ii. *e.*
	23	18	ii. O.
20	0	25	i. *e.*
	3	17	i. O.
	22	8	i. *t.*
21	0	28	i. T.
	4	41	iii. *e.*
	10	34	iii. O.
	15	23	ii. *t.*
	18	18	ii. T.
	18	54	i. *e.*
	21	47	i. O.
22	2	1	iv. *t.*
	6	15	iv. T.
	16	38	i. *t.*
	18	58	i. T.
23	8	33	ii. *e.*
	12	43	ii. O.
	13	22	i. *e.*
	16	17	i. O.
24	11	8	i. *t.*
	13	28	i. T.
	21	18	iii. *t.*
25	0	52	iii. T.
	4	46	ii. *t.*
	7	42	ii. T.
	7	50	i. *e.*
	10	47	i. O.
26	5	38	i. *t.*
	7	58	i. T.
	21	52	ii. *e.*
27	2	8	ii. O.
	2	19	i. *e.*
	5	17	i. O.
28	0	9	i. *t.*
	2	28	i. T.
	8	40	iii. *e.*
	14	58	iii. O.
	18	9	ii. *t.*
	20	47	i. *e*
	21	5	ii. T.
	23	47	i. O.
29	18	39	i. t.
	20	58	i. T.
30	5	39	iv. e.
	9	42	iv. E.

Aug. (con.).

d	h	m	
30	11	10	ii. *e.*
	12	31	iv. *o.*
	15	16	i. *e.*
	15	33	ii. O.
	16	50	iv. O.
	18	16	i. O.
31	13	9	i. *t.*
	15	28	i. T.

September.

d	h	m	
1	1	42	iii. *t.*
	5	17	iii. T.
	7	32	ii. *t.*
	9	44	i. *e.*
	10	28	ii. T.
	12	46	i. O.
2	7	39	i. *t.*
	9	58	i. T.
3	0	28	ii. *e.*
	4	12	i. *e.*
	4	57	ii. O.
	7	16	i. O.
4	2	9	i. *t.*
	4	28	i. T.
	12	38	iii. *e.*
	19	22	iii. O.
	20	54	ii. *t.*
	22	41	i. *e.*
	23	51	ii. T.
5	1	46	i. O.
	20	38	i. *t.*
	22	58	i. T.
6	13	46	ii. *e.*
	17	9	i. *e.*
	18	21	ii. O.
	20	16	i. O.
7	15	8	i. *t.*
	17	28	i. T.
	22	11	iv. *t.*
8	2	35	iv. T.
	6	3	iii. *t.*
	9	39	iii. T.
	10	17	ii. *t.*
	11	37	i. *e.*
	13	13	ii. T.
	14	46	i. O.
9	9	38	i. *t.*
	11	58	i. T.
10	3	5	ii. *e.*
	6	6	i. *e.*
	7	45	ii. O.
	9	15	i. O.
11	4	8	i. *t.*
	6	28	i. T.
	16	37	iii. *e.*
	20	1	iii. E.
	20	7	iii. *o.*
	23	39	ii. *t.*
	23	44	iii. O.
12	0	34	i. *e.*
	2	35	ii. T.
	3	45	i. O.
	22	38	i. *t.*

Sept. (con.).

d	h	m	
13	0	58	i. T.
	16	22	ii. *e.*
	19	2	i. *e.*
	21	9	ii. O.
	22	15	i. O.
14	17	8	i. *t.*
	19	28	i. T.
15	10	23	iii. *t.*
	13	1	ii. *t.*
	13	31	i. *e.*
	14	1	iii. T.
	15	57	ii. T.
	16	44	i. O.
	23	39	iv. *e.*
16	3	48	iv. E.
	8	37	iv. *o.*
	11	37	i. *t.*
	13	5	iv. O.
	13	57	i. T.
17	5	41	ii. *e.*
	7	59	i. *e.*
	10	33	ii. O.
	11	14	i. O.
18	6	7	i. *t.*
	8	27	i. T.
	20	35	iii. *e.*
	23	59	iii. E.
19	0	27	iii. *o.*
	2	23	ii. *t.*
	2	27	i. *e.*
	4	4	iii. O.
	5	19	ii. T.
	5	43	i. O.
20	0	37	i. *t.*
	2	56	i. T.
	18	59	ii. *e.*
	20	56	i. *e.*
	23	56	ii. O.
21	0	13	i. O.
	19	7	i. *t.*
	21	26	i. T.
22	14	42	iii. *t.*
	15	24	i. *e.*
	15	44	ii. *t.*
	18	20	iii. T.
	18	40	ii. T.
	18	42	i. O.
23	13	36	i. *t.*
	15	56	i. T.
24	8	18	ii. *e.*
	9	52	i. *e.*
	13	12	i. O.
	13	19	ii. O.
	18	1	iv. *t.*
	22	32	iv. T.
25	8	6	i. *t.*
	10	26	i. T.
26	0	33	iii *e.*
	3	58	iii. E.
	4	21	i. *e.*
	4	44	iii. *o.*
	5	5	ii. *t.*
	7	41	i. O.
	8	2	ii. T.

Sept. (con.).

d	h	m	
26	8	22	iii. O.
27	2	35	i. *t.*
	4	55	i. T.
	21	35	ii. *e.*
	22	49	i. *e.*
28	2	11	i. O.
	2	42	ii. O.
	21	5	i. *t.*
	23	25	i. T.
29	17	17	i. *e.*
	18	26	ii. *t.*
	18	58	iii. *t.*
	20	40	i. O.
	21	22	ii. T.
	22	36	iii. T.
30	15	34	i. *t.*
	17	54	i. T.

October.

d	h	m	
1	10	54	ii. *e.*
	11	45	i. *e.*
	15	9	i. O.
	16	4	ii. O.
2	10	4	i. *t.*
	12	24	i. T.
	17	39	iv. *e.*
	21	53	iv. E.
3	4	17	iv. *o.*
	4	30	iii. *e.*
	6	14	i. *e.*
	7	46	ii. *t.*
	7	56	iii. E.
	8	52	iv. O.
	8	58	iii. *o.*
	9	39	i. O.
	10	43	ii. T.
	12	37	iii. O.
4	4	33	i. *t.*
	6	53	i. T.
5	0	12	ii. *e.*
	0	42	i. *e.*
	4	8	i. O.
	5	26	ii. O.
	23	2	i. *t.*
6	1	22	i. T.
	19	10	i. *e.*
	21	7	ii. *t.*
	22	37	i. O.
	23	12	iii. *t.*
7	0	3	ii. T.
	2	52	iii. T.
	17	32	i. *t.*
	19	52	i. T.
8	13	30	ii. *e.*
	13	39	i. *e.*
	17	6	i. O.
	18	48	ii. O.
9	12	1	i. *t.*
	14	21	i. T.
10	8	7	i. *e.*
	8	28	iii. *e.*
	10	26	ii. t.
	11	35	i. O.

Oct. (con.).

d	h	m	
10	11	54	iii.
	13	10	iii.
	13	23	ii.
	16	50	iii.
11	6	30	i. *t.*
	8	50	i. T.
	13	19	iv. *t*
	17	57	iv.
12	2	35	i. *e.*
	2	48	ii. *e.*
	6	4	i. O.
	8	9	ii. O
13	0	59	i. *t.*
	3	19	i. T.
	21	3	i. *e.*
	23	46	ii. *t.*
14	0	33	i. O.
	2	42	ii. T
	3	23	iii. *t*
	7	3	iii.
	19	28	i. *t.*
	21	48	i. T.
15	15	32	i. *e.*
	16	7	ii. *e.*
	19	2	i. O.
	21	30	ii. O
16	13	58	i. *t.*
	16	18	i. T.
17	10	0	i. *e.*
	12	26	iii. *e*
	13	5	ii. *t.*
	13	31	i. O.
	15	53	iii.
	16	1	ii. T
	17	18	iii.
	20	58	iii.
18	8	26	i. *t.*
	10	46	i. T.
19	4	28	i. *e.*
	5	24	ii. *e.*
	8	0	i. O.
	10	51	ii. O
	11	39	iv. *e.*
	15	57	iv.
	23	23	iv. *o*
20	2	56	i. *t.*
	4	4	iv. O
	5	16	i. T.
	22	56	i. *e.*
21	2	23	ii. *t.*
	2	29	i. O.
	5	20	ii. T
	7	30	iii. *t*
	11	11	iii.
	21	24	i. *t.*
	23	44	i. T.
22	17	25	i. *e.*
	18	43	ii. *e.*
	20	57	i. O.
23	0	11	ii. O
	15	53	i. *t.*
	18	13	i. T.
24	11	53	i. e.
	15	26	i. O

Oct. (con.).

d	h	m	
24	15	42	ii. *t.*
	16	23	iii. *e.*
	18	38	ii. T.
	19	51	iii. E.
	21	24	iii. *o.*
25	1	4	iii. O.
	10	22	i. *t.*
	12	42	i. T.
26	6	21	i. *e.*
	8	0	ii. *e.*
	9	55	i. O.
	13	30	ii. O.
27	4	51	i. *t.*
	7	11	i. T.
28	0	50	i. *e.*
	4	23	i. O.
	5	0	ii. *t.*
	7	56	ii. T.
	7	59	iv. *t.*
	11	34	iii. *t.*
	12	43	iv. T.
	15	15	iii. T.
	23	19	i. *t.*
29	1	40	i. T.
	19	18	i. *e.*
	21	19	ii. *e.*
	22	52	i. O.
30	2	49	ii. O.
	17	48	i. *t.*
	20	8	i. T.
31	13	46	i. *e.*
	17	20	i. O.
	18	17	ii. *t.*
	20	22	iii. *e.*
	21	13	ii. T.
	23	50	iii. E.

November.

d	h	m	
1	1	25	iii. *o.*
	5	7	iii. O.
	12	17	i. *t.*
	14	37	i. T.
2	8	14	i. *e.*
	10	37	ii. *e.*
	11	49	i. O.
	16	8	ii. O.
3	6	45	i. *t.*
	9	5	i. T.
4	2	43	i. *e.*
	6	17	i. O.
	7	34	ii. *t.*
	10	30	ii. T.

Nov. (con.).

d	h	m	
4	15	33	iii. *t.*
	19	14	iii. T.
5	1	13	i. *t.*
	3	34	i. T.
	5	38	iv. *e.*
	10	1	iv. E.
	17	43	iv. *o.*
	21	11	i. *e.*
	22	29	iv. O.
	23	55	ii. *e.*
6	0	45	i. O.
	5	26	ii. O.
	19	42	i. *t.*
	22	2	i. T.
7	15	39	i. *e.*
	19	13	i. O.
	20	51	ii. *t.*
	23	47	ii. T.
8	0	20	iii. *e.*
	3	48	iii. E.
	5	23	iii. *o.*
	9	5	iii. O.
	14	10	i. *t.*
	16	30	i. T.
9	10	7	i. *e.*
	13	13	ii. *e.*
	13	41	i. O.
	18	43	ii. O.
10	8	38	i. *t.*
	10	59	i. T.
11	4	36	i. *e.*
	8	9	i. O.
	10	6	ii. *t.*
	13	3	ii. T.
	19	29	iii. *t.*
	23	11	iii. T.
12	3	7	i. *t.*
	5	27	i. T.
	23	4	i. *e.*
13	2	31	ii. *e.*
	2	37	i. O.
	8	0	ii. O.
	21	35	i. *t.*
	23	55	i. T.
14	1	52	iv. *t.*
	6	39	iv. T.
	17	32	i. *e.*
	21	5	i. O.
	23	22	ii. *t.*
15	2	18	ii. T.
	4	18	iii. *e.*
	7	46	iii. E.
	9	17	iii. *o.*

Nov. (con.).

d	h	m	
15	12	59	iii. O.
	16	3	i. *t.*
	18	23	i. T.
16	12	0	i. *e.*
	15	33	i. O.
	15	49	ii. *e.*
	21	17	ii. O.
17	10	31	i. *t.*
	12	51	i. T.
18	6	29	i. *e.*
	10	1	i. O.
	12	37	ii. *t.*
	15	33	ii. T.
	23	20	iii. *t.*
19	3	2	iii. T.
	4	59	i. *t.*
	7	19	i. T.
20	0	57	i. *e.*
	4	29	i. O.
	5	7	ii. *e.*
	10	32	ii. O.
	23	26	i. *t.*
21	1	46	i. T.
	19	25	i. *e.*
	22	56	i. O.
	23	38	iv. *e.*
22	1	51	ii. *t.*
	4	4	iv. E.
	4	48	ii. T.
	8	15	iii. *e.*
	11	10	iv. *o.*
	11	44	iii. E.
	13	5	iii. *o.*
	15	58	iv. O.
	16	47	iii. O.
	17	54	i. *t.*
	20	14	i. T.
23	13	53	i. *e.*
	17	24	i. O.
	18	25	ii. *e.*
	23	47	ii. O.
24	12	22	i. *t.*
	14	42	i. T.
25	8	22	i. *e.*
	11	52	i. O.
	15	5	ii. *t.*
	18	1	ii. T.
26	3	8	iii. *t.*
	6	49	i. *t.*
	6	50	iii. T.
	9	10	i. T.
27	2	50	i. *e.*
	6	19	i. O.

Nov. (con.).

d	h	m	
27	7	43	ii. *e.*
	13	2	ii. O.
28	1	17	i. *t.*
	3	37	i. T.
	21	18	i. *e.*
29	0	46	i. O.
	4	18	ii. *t.*
	7	15	ii. T.
	12	12	iii. *e.*
	15	42	iii. E.
	16	50	iii. *o.*
	19	44	i. *t.*
	20	32	iii. O.
	22	5	i. T.
30	15	47	i. *e.*
	18	49	iv. *t.*
	19	14	i. O.
	21	0	ii. *e.*
	23	39	iv T.

December.

d	h	m	
1	2	16	ii. O.
	14	12	i. *t.*
	16	32	i. T.
2	10	15	i. *e.*
	13	41	i. O.
	17	31	ii. *t.*
	20	27	ii. T.
3	6	50	iii. *t.*
	8	39	i. *t.*
	10	33	iii. T.
	10	59	i. T.
4	4	43	i. *e.*
	8	8	i. O.
	10	19	ii. *e.*
	15	29	ii. O.
5	3	6	i. *t.*
	5	26	i. T.
	23	11	i. *e.*
6	2	35	i. O.
	6	43	ii. *t.*
	9	40	ii. T.
	16	10	iii. *e.*
	19	40	iii. E.
	20	29	iii. *o.*
	21	33	i. *t.*
	23	53	i. T.
7	0	11	iii. O.
	17	40	i. *e.*
	21	2	i. O.
	23	36	ii. *e.*

Dec. (con.).

d	h	m	
8	4	42	ii. O.
	16	0	i. *t.*
	17	37	iv. *e.*
	18	21	i. T.
	22	8	iv. E.
9	3	38	iv. *o.*
	8	28	iv. O.
	12	8	i. *e.*
	15	29	i. O.
	19	55	ii. *t.*
	22	51	ii. T.
10	10	27	i. *t.*
	10	28	iii. *t.*
	12	48	i. T.
	14	10	iii. T.
11	6	36	i. *e.*
	9	56	i. O.
	12	55	ii. *e.*
	17	54	ii. O.
12	4	54	i. *t.*
	7	15	i. T.
13	1	5	i. *e.*
	4	23	i. O.
	9	6	ii. *t.*
	12	2	ii. T.
	20	8	iii. *e.*
	23	21	i. *t.*
	23	39	iii. E.
14	0	4	iii. *o.*
	1	42	i. T.
	3	46	iii. O.
	19	33	i. *e.*
	22	50	i. O.
15	2	12	ii. *e.*
	7	5	ii. O.
	17	48	i. *t.*
	20	8	i. T.
16	14	1	i. *e.*
	17	16	i. O.
	22	17	ii. *t.*
17	1	13	ii. T.
	10	48	iv. *t.*
	12	15	i. *t.*
	14	0	iii. *t.*
	14	35	i. T.
	15	39	iv. T.
	17	42	iii. T.
18	8	30	i. *e.*
	11	43	i. O.
	15	30	ii. *e.*
	20	16	ii. O.
19	6	42	i. *t.*
	9	2	i. T.

Dec. (con

d	h	m
20	2	58
	6	10
	11	27
	14	23
21	0	7
	1	8
	3	29
	7	17
	21	26
22	0	36
	4	48
	9	27
	19	35
	21	55
23	15	55
	19	3
24	0	37
	3	33
	14	1
	16	22
	17	28
	21	11
25	10	23
	11	37
	13	29
	16	10
	18	6
	19	6
	22	37
	23	57
26	8	28
	10	48
27	4	52
	7	56
	13	46
	16	42
28	2	54
	4	5
	5	15
	10	43
	23	20
29	2	22
	7	23
	11	46
	21	21
	23	41
30	17	48
	20	48
31	2	55
	5	51
	15	47
	18	7
	20	53

The satellites *reappear* after eclipse, and III. and IV. *disappear* on the *f* side from Janu to June. They *disappear*, and III. and IV. *reappear* after eclipse on the *p* side from August December.

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Jan. 12^{h}	Feb. 11^{h}	March. 10^{h}	April. 10^{h}	May. $9\frac{1}{2}^{h}$	June. 9^{h}	Aug. $15\frac{1}{2}^{h}$	Sept. $15\frac{1}{2}^{h}$	Oct. 15^{h}	Nov. $14\frac{1}{2}^{h}$	Dec. 14^{h}	Day.
1	431O2	2O41●	23O4●	♃1O43	♃42O3	432O●		32O14	34O●●	♃42O3	4O123	1
2	43O21	41O23	1O234	2O134	43O12	4321O		321O4	341O2	4O13●	4O123	2
3	4231O	4O213	O1234	13O24	♃341O	4O12●		3O124	2O341	41O23	4231O	3
4	4O123	421O3	21O34	3O124	324O1	♃41O3		1O24●	21O34	42O31	43O1●	4
5	4O23●	♃43O2	2O314	321O4	13O24	42O13		2O134	O1234	3214O	431O2	5
6	421O3	43O12	31O42	32O14	O1234	41O23		1O43●	1O234	3O124	432O1	6
7	♃24O1	4321O	34O21	1O324	21O34	3O412		♃O432	23O14	31O24	421O3	7
8	31O24	423O1	432O●	♃O243	2O134	321O4		432O1	3O4●●	♃2O34	4O123	8
9	3O214	14O23	41O●●	♃2O13	3O24●	♃32O4		4321O	31O24	O134●	O423●	9
10	231O4	O2143	4O123	♃41O2	31O24	3O124		43O12	2O14●	1O234	♃21O4	10
11	O134●	21O34	421O3	43O12	32O14	1O234		413O2	♃21O3	2O314	3O14●	11
12	1O234	3O14●	42O31	4321O	13O4●	2O134		42O13	4O213	321O4	31O24	12
13	♃2O34	3O24●	431O2	432O1	4O132	1O34●		412O3	41O23	3O124	32O14	13
14	2O314	321O4	34O21	41O32	421O3	3O124	♃1O34	4O123	423O1	341O2	21O34	14
15	31O42	23O14	321O4	♃4O23	42O13	321O4	♃2O34	♃43O●	4321O	42O31	O1234	15
16	34O21	1O234	♃O4●●	42O3●	♃4O2●	324O1	O1234	321O4	♃43O2	42O3●	1O243	16
17	4231O	O2143	O1234	14O3●	431O2		1O324	3O214	♃4O1●	41O23	♃♃♃2O	17
18	42O13	214O3	12O34	3O142	432O1		32O14	31O24	421O3	♃4O31	342O1	18
19	41O23	43O1●	2O134	312O4	431O●		312O4	2O134	O213●	4231O	431O2	19
20	♃42O3	43O2●	31O24	32O14	4O312		3O124	12O34	1O423	43O21	♃43O1	20
21	42O13	4321O	3O214	1O24●	12O3●		♃1O34	O1234	23O14	341O2	421O3	21
22	431O2	423O1	321O4	O1234	2O143		24O13	♃O24●	321O4	2O1●●	4O213	22
23	34O12	41O23	♃32O4	2O34●	1O324		4O23●	♃32O4	3O124	2O43●	41O23	23
24	321O●	4O213	4O23●	1O34●	♃3O24		41O32	3O421	3O24●	♃O234	♃42O3	24
25	2O314	421O3	412O3	3O142	32O14		432O1	431O2	21O34	O2134	32O1●	25
26	1O234	♃24O1	42O13	3124O	312O4		4312O	42O31	O2134	231O4	31O42	26
27	2O134	31O24	413O2	432O1	O3124		43O12	412O3	1O423	3O214	♃3O14	27
28	2O34●	♃32O4	43O12	41O2●	12O43		41O32	4O123	♃24O1	31O24	21O34	28
29	31O24		4321O	4O123	2O413		24O13	41O32	3421O	2O14●	O2134	29
30	3O124		432O1	421O3	41O32		O3●●●	423O1	43O12	21O43	1O234	30
31	321O4		4O32●		43O12		1O324		43O2●		2O134	31

The circle (O) represents Jupiter; ♃ signifies that the satellite is on the disk; ● signifies that the satellite is behind the disk or in the shadow.

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49 3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 46·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 19·3	67	2 15·2	77	4 4·9		

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1870, observed	19 53 west.	0·1782	67 52
1880, "	18 33 "	0·1804	67 36
1890, "	17 29 "	0·1823	67 23
1900, "	16 29 "	0·1844	67 8
1907, inferred	16 6 "	0·1860	66 56

The horizontal force is given in C. G. S. measure.

SATELLITES OF SATURN.

East Elongations.

MIMAS.

May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
12	16·6	1	11·7	1	15·4	1	17·8	1	20·0	1	1·0	1	3·3	1	7·2
13	15·3	2	10·3	2	14·1	2	16·4	2	18·6	1	23·6	2	1·9	2	5·8
14	13·9	3	8·9	3	12·7	3	15·0	3	17·2	2	22·2	3	0·6	3	4·4
15	12·5	4	7·5	4	11·3	4	13·6	4	15·8	3	20·8	3	23·2	4	3·0
16	11·1	5	6·1	5	9·9	5	12·2	5	14·4	4	19·5	4	21·8	5	1·6
17	9·7	6	4·8	6	8·6	6	10·8	6	13·1	5	18·1	5	20·4	6	0·3
18	8·4	7	3·4	7	7·2	7	9·4	7	11·7	6	16·7	6	19·0	6	22·9
19	7·0	8	2·0	8	5·8	8	8·1	8	10·3	7	15·3	7	17·7	7	21·5
20	5·6	9	0·6	9	4·4	9	6·7	9	8·9	8	13·9	8	16·3	8	20·1
21	4·2	9	23·2	10	3·0	10	5·3	10	7·5	9	12·5	9	14·9	9	18·7
22	2·8	10	21·9	11	1·6	11	4·0	11	6·1	10	11·2	10	13·5	10	17·4
23	1·5	11	20·5	12	0·2	12	2·6	12	4·8	11	9·8	11	12·1	11	16·0
24	0·1	12	19·1	12	22·8	13	1·2	13	3·4	12	8·4	12	10·8	12	14·6
24	22·7	13	17·7	13	21·4	13	23·8	14	2·0	13	7·0	13	9·4	13	13·2
25	21·3	14	16·3	14	20·1	14	22·4	15	0·6	14	5·6	14	8·0	14	11·8
26	19·9	15	14·9	15	18·7	15	21·0	15	23·2	15	4·2	15	6·6	15	10·5
27	18·6	16	13·6	16	17·3	16	19·6	16	21·8	16	2·8	16	5·2	16	9·1
28	17·2	17	12·2	17	15·9	17	18·2	17	20·4	17	1·4	17	3·9	17	7·7
29	15·8	18	10·8	18	14·5	18	16·8	18	19·0	18	0·1	18	2·5	18	6·3
30	14·4	19	9·4	19	13·1	19	15·4	19	17·7	18	22·7	19	1·1	19	5·0
31	13·0	20	8·0	20	11·7	20	14·0	20	16·3	19	21·3	19	23·7	20	3·6
		21	6·7	21	10·4	21	12·6	21	14·9	20	19·9	20	22·3	21	2·2
		22	5·3	22	9·0	22	11·2	22	13·5	21	18·5	21	21·0	22	0·8
		23	3·9	23	7·6	23	9·8	23	12·1	22	17·2	22	19·6	22	23·5
		24	2·5	24	6·2	24	8·5	24	10·7	23	15·8	23	18·2	23	22·1
		25	1·1	25	4·8	25	7·1	25	9·3	24	14·4	24	16·8	24	20·7
		25	23·7	26	3·4	26	5·7	26	8·0	25	13·0	25	15·4	25	19·3
		26	22·4	27	2·1	27	4·3	27	6·6	26	11·6	26	14·1	26	17·9
		27	21·0	28	0·7	28	2·9	28	5·2	27	10·2	27	12·7	27	16·6
		28	19·6	28	23·3	29	1·6	29	3·8	28	8·9	28	11·3	28	15·2
		29	18·2	29	21·9	30	0·2	30	2·4	29	7·5	29	9·9	29	13·8
		30	16·8	30	20·5	30	22·8			30	6·1	30	8·5	30	12·4
				31	19·1	31	21·4			31	4·7			31	11·0

ENCELADUS.

May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
12	0·8	1	14·2	1	17·7	2	6·0	1	9·3	1	12·5	2	0·8	2	4·2
13	9·7	2	23·1	3	2·6	3	14·9	2	18·1	2	21·2	3	9·6	3	13·1
14	18·6	4	8·0	4	11·5	4	23·7	4	3·0	4	6·3	4	18·5	4	22·0
16	3·5	5	16·9	5	20·4	6	8·6	5	11.9	5	15·1	6	3·4	6	6·9
17	12·4	7	1·8	7	5·3	7	17·5	6	20·8	7	0·0	7	12·3	7	15·8
18	21·3	8	10·7	8	14·1	9	2·4	8	5·6	8	8·9	8	21·2	9	0·7
20	6·2	9	19·5	9	23·0	10	11·3	9	14·5	9	17·7	10	6·0	10	9·6
21	15·1	11	4·4	11	7·9	11	20·1	10	23·4	11	2·6	11	14·9	11	18·4
23	0·0	12	13·3	12	16·8	13	5·0	12	8·2	12	11·5	12	23·8	13	3·3
24	8·9	13	22·2	14	1·7	14	13·9	13	17·1	13	20·4	14	8·7	14	12·2
25	17·8	15	7·1	15	10·6	15	22·8	15	2·0	15	5·3	15	17·6	15	21·1
27	2·7	16	16·0	16	19·5	17	7·6	16	10·8	16	14·2	17	2·4	17	6·0
28	11·5	18	0·9	18	4·3	18	16·5	17	19·7	17	23·1	18	11·3	18	14·9
29	20·4	19	9·8	19	13·2	20	1·4	19	4·6	19	7·9	19	20·2	19	23·8
31	5·3	20	18·7	20	22·1	21	10·2	20	13·4	20	16·8	21	5·1	21	8·7
		22	3·5	22	6·9	22	19·1	21	22·3	22	1·7	22	14·0	22	17·6
		23	12·4	23	15·8	24	4·0	23	7·2	23	10·6	23	22·9	24	2·5
		24	21·3	25	0·7	25	12·8	24	16·1	24	19·5	25	7·8	25	11·4
		26	6·2	26	9·6	26	21·7	26	1·0	26	4·3	26	16·7	26	20·3
		27	15·1	27	18·5	28	6·6	27	9·9	27	13·2	28	1·6	28	5·2
		29	0.0	29	3·3	29	15·5	28	18·7	28	22·1	29	10·4	29	14·1
		30	8·8	30	12·2	31	0·4	30	3·6	30	7·0	30	19·3	30	23·0
				31	21·1					31	15·9				

East Elongations.

TETHYS.

May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
12	18·5	2	13·1	2	18·0	1	22·8	1	3·5	1	8·1	2	10·1	2	15·0
14	15·9	4	10·4	4	15·3	3	20·1	3	0·8	3	5·4	4	7·4	4	12·3
16	13·2	6	7·7	6	12·6	5	17·4	4	22·0	5	2·6	6	4·7	6	9·6
18	10·5	8	5·0	8	10·0	7	14·7	6	19·3	6	23·9	8	2·0	8	6·9
20	7·8	10	2·3	10	7·3	9	12·0	8	16·6	8	21·2	9	23·3	10	4·3
22	5·2	11	23·7	12	4·6	11	9·3	10	13·9	10	18·5	11	20·6	12	1·6
24	2·5	13	21·0	14	1·9	13	6·5	12	11·2	12	15·8	13	17·9	13	22·9
25	23·8	15	18·3	15	23·2	15	3·9	14	8·5	14	13·1	15	15·2	15	20·2
27	21·1	17	15·6	17	20·4	17	1·2	16	5·8	16	10·4	17	12·5	17	17·5
29	18·4	19	12·9	19	17·7	18	22·4	18	3·0	18	7·7	19	9·8	19	14·8
31	15·7	21	10·2	21	15·0	20	19·7	20	0·3	20	5·0	21	7·1	21	12·2
		23	7·5	23	12·3	22	17·0	21	21·6	22	2·3	23	4·4	23	9·5
		25	4·8	25	9·6	24	14·3	23	18·9	23	23·6	25	1·8	25	6·8
		27	2·1	27	6·9	26	11·6	25	16·2	25	20·9	26	23·1	27	4·1
		28	23·4	29	4·2	28	8·9	27	13·5	27	18·2	28	20·4	29	1·5
		30	20·7	31	1·5	30	6·2	29	10·8	29	15·5	30	17·7	30	22·8
										31	12·8				

DIONE.

May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
13	15·2	1	19·2	1	21·9	1	0·2	2	20·1	2	22·2	2	0·4	2	2·9
16	8·9	4	12 9	4	15·6	3	17·9	5	13·7	5	15·8	4	18·1	4	20·6
19	2·6	7	6·6	7	9·2	6	11·6	8	7·3	8	9·5	7	11·7	7	14·3
21	20·3	10	0·3	10	2·9	9	5·2	11	1·0	11	3·1	10	5·4	10	8·0
24	14·0	12	18·0	12	20·6	11	22·9	13	18·6	13	20·8	12	23·1	13	1·7
27	7·7	15	11·7	15	14·3	14	16·5	16	12·3	16	14·4	15	16·8	15	19·4
30	1·4	18	5·4	18	7·9	17	10·2	19	5·9	19	8·1	18	10·5	18	13·1
		20	23·1	21	1·6	20	3·8	21	23·6	22	1·7	21	4·1	21	6·8
		23	16·8	23	19·2	22	21·5	24	17·2	24	19·4	23	21·8	24	0·6
		26	10·5	26	12·9	25	15·1	27	10·9	27	13·1	26	15·5	26	18·3
		29	4·2	29	6·6	28	8·8	30	4·5	30	6·7	29	9·2	29	12·0
						31	2·4								

RHEA.

May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
16	6·8	3	8·8	5	0·0	1	2·3	1	16·6	3	6·8	3	21·2	5	12·1
20	19·3	7	21·3	9	12·4	5	14·6	6	4·9	7	19·1	8	9·5	10	0·5
25	7·8	12	9·7	14	0·8	10	3·0	10	17·3	12	7·4	12	21·9	14	13·0
29	20·3	16	22·2	18	13·2	14	15·3	15	5·5	16	19·7	17	10·3	19	1·5
		21	10·7	23	1·6	19	3·6	19	17·8	21	8·1	21	22·8	23	13·9
		25	23·1	27	13·9	23	16·0	24	6·1	25	20·4	26	11·2	28	2·4
		30	11·6			28	4·3	28	18·4	30	8·8	30	23·6		

TITAN.

May.		June.		July.		Aug.		Sept.		Oct.		Nov.		Dec.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
26	13·6	11	13·1	13	11·2	14	7·1	15	2·1	16	20·9	1	18·7	3	15·8
		27	*12·3*	*29*	*9·2*	30	4·9	30	23·5			17	17·1	19	14·9

East Elongations.

HYPERION.

May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
d h	d h	d h	d h	d h	d h	d h	d h
20 14·2	11 1·2	2 10·8 23 19·9	14 3·1	4 9·6 25 14·9	16 20·2	7 1·7 28 7·9	19 14·9

IAPETUS.

	d h	d h	d h
E. Elongation	May 24 21·4,	Aug. 12 8·6,	Oct. 29 12·0
Inf. Conjunction	June 14 9·1,	Sept. 1 8·2,	Nov. 18 15·8
W. Elongation	July 4 15·4,	Sept. 21 2·2,	Dec. 8 19·0
Sup. Conjunction	July 24 0·0,	Oct. 10 3·8,	Dec. 28 8·2

APPARENT ELEMENTS OF SATURN'S RINGS.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring.		Latitude above Plane of Ring.	
		Maj. Axis.	Min. Axis.	Earth.	Sun.
	° ′	″	″	° ′	° ′
Jan. 14	5 36·0	37·54	+3·15	4 49·2 N.	2 51·9 N.
Feb. 3	5 27·0	36·80	2·45	3 49·3 N.	2 34·2 N.
23	5 16·6	36·38	1·72	2 42·4 N.	2 16·5 N.
Mar. 15	5 5·3	36·32	0·99	1 33·5 N.	1 58·8 N.
April 4	4 53·9	36·60	+0·29	0 26·9 N.	1 41·2 N.
24	4 43·2	37·21	−0·36	0 33·4 S.	1 23·4 N.
May 4	4 38·2	37·63	0·65	0 59·9 S.	1 14·5 N.
14	4 33·7	38·13	0·92	1 23·3 S.	1 5·6 N.
24	4 29·7	38·69	1·16	1 43·3 S.	0 56·7 N.
June 3	4 26·4	39·31	1·37	1 59·5 S.	0 47·8 N.
13	4 23·8	39·97	1·53	2 11·5 S.	0 38·9 N.
23	4 21·8	40·67	1·65	2 19·2 S.	0 30·0 N.
July 3	4 20·7	41·39	1·71	2 22·1 S.	0 21·1 N.
13	4 20·5	42·11	1·72	2 20·4 S.	0 12·2 N.
23	4 21·1	42·80	1·67	2 14·0 S.	0 3·3 N.
Aug. 2	4 22·6	43·43	1·56	2 3·3 S.	0 5·7 S.
12	4 24·8	43·99	1·39	1 48·6 S.	0 14·7 S.
22	4 27·7	44·45	1·17	1 30·8 S.	0 23·7 S.
Sept. 1	4 31·1	44·78	0·90	1 10·3 S.	0 32·7 S.
11	4 34·7	44·97	0·63	0 48·5 S.	0 41·6 S.
21	4 38·5	44·99	0·34	0 26·3 S.	0 50·5 S,
Oct. 1	4 42·2	44·86	−0·06	0 5·0 S.	0 59·5 S.
11	4 45·6	44·58	+0·19	0 14·3 N.	1 8·5 S.
21	4 48·7	44·16	0·40	0 30·8 N.	1 17·4 S.
31	4 51·1	43·62	0·55	0 43·3 N.	1 26·4 S.
Nov. 10	4 52·8	43·00	0·64	5 51·5 N.	1 35·4 S.
20	4 53·7	42·32	0·68	0 54·6 N.	1 44·4 S.
30	4 53·7	41·60	0·64	0 52·7 N.	1 53·4 S.
Dec. 10	4 52·9	40·88	0·54	0 45·6 N.	2 2·4 S.
20	4 51·2	40·17	0·39	0 33·7 N.	2 11·4 S.
30	4 48·8	39·50	+0·19	0 17·4 N.	2 20·4 S.

+ *denotes* North surface visible ; — South surface.

SATELLITES OF URANUS.

North Elongations.

ARIEL.

April.		May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
24	18·0	2	7·5	1	13·4	1	19·4	1	1·4	2	19·9	3	1·9
27	6·5	4	20·0	4	1·9	4	7·9	3	13·9	5	8·4	5	14·4
29	19·0	7	8·5	6	14·4	6	20·4	6	2·4	7	20·9	8	2·9
		9	20·9	9	2·9	9	8·9	8	14·9	10	9·4	10	15·4
		12	9·4	11	15·4	11	21·4	11	3·4	12	21·9	13	3·9
		14	21·9	14	3·9	14	9·9	13	15·9	15	10·4	15	16·4
		17	10·4	16	16·4	16	22·4	16	4·4	17	22·9	18	4·9
		19	22·9	19	4·9	19	10·9	18	16·9	20	11·4	20	17·4
		22	11·4	21	17·4	21	23·4	21	5·4	22	23·9	23	5·9
		24	23·9	24	5·9	24	11·9	23	17·9	25	12·4		
		27	12·4	26	18·4	27	0·4	26	6·4	28	0·9		
		30	0·9	29	6·9	29	12·9	28	18·9	30	13·4		
								31	7·4				

UMBRIEL.

April.		May.		June.		July.		August.		September.		October.		November.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
19	14·0	2	0·4	4	4·2	3	4·6	1	5·1	3	9·0	2	9·3	4	12·9
23	17·5	6	3·9	8	7·6	7	8·1	5	8·6	7	12·5	6	12·8		
27	20·9	10	7·4	12	11·1	11	11·6	9	12·1	11	16·0	10	16·2		
		14	10·8	16	14·6	15	15·1	13	15·6	15	19·4	14	19·7		
		18	14·3	20	18·1	19	18·5	17	19·1	19	22·9	18	23·1		
		22	17·7	24	21·6	23	22·0	21	22·6	24	2·4	23	2·6		
		26	21·2	29	1·1	28	1·6	26	2·1	28	5·9	27	6·0		
		31	0·7					30	5·5			31	9·5		

TITANIA.

April.		May.		June.		July.		August.		September.		October.		November.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
17	21·6	5	7·5	9	3·6	5	6·9	9	3·3	4	6·4	9	2·2	4	4·7
26	14·6	14	0·5	17	20·7	14	0·0	17	20·3	12	23·4	17	19·1	12	21·5
		22	17·5	26	13·8	22	17·1	26	13·4	21	16·4	26	11·9		
		31	10·6			31	10·2			30	9·3				

OBERON.

May.		June.		July.		August.		September.		October.	
d	h	d	h	d	h	d	h	d	h	d	h
20	11·1	2	22·4	13	8·8	9	7·7	5	6·5	2	4·7
		16	9·8	26	20·3	22	19·1	18	17·7	15	15·7
		29	21·3								

Apparent Apsides.

	Position-angle.	Apparent Distances.			
		Ariel.	Umbriel.	Titania.	Oberon.
	°	″	″	″	″
April 14	192·4	13·7	19·1	31·4	42·0
July 3	194·5	14·3	20·0	32·7	43·8
Sept. 21	196·8	13·7	19·1	31·4	41·7

SATELLITE OF NEPTUNE.

Times of passage through the east apse of apparent orbit.

January.		February.		March.		April.		September.		October.		November.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h	d	h
5	22·9	4	8·6	5	18·1	4	3·4	3	20·3	3	5·2	1	14·4	6	20·9
11	20·0	10	5·7	11	15·1	10	0·4	9	17·3	9	2·2	7	11·4	12	18·0
17	17·2	16	2·8	17	12·2	15	21·4	15	14·3	14	23·3	13	8·5	18	15·1
23	14·3	21	23·9	23	9·3	21	18·4	21	11·3	20	20·3	19	5·6	24	12·2
29	*11·4*	*27*	*21·0*	29	6·3			27	8·2	26	17·3	25	2·7	30	9·4
												30	23·8		

Position-angle and distance of the apse:—Jan. 2, 274°·1, 16″·8; *April 12, 272°·4, 16″·2*; Sept. 3, 277°·9, 16″·0; Dec. 32, 276°·9, 16″·8.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1907.	P.	D.	L.	1907.	P.	D.	L.
	° ′	° ′	° ′		° ′	° ′	° ′
Jan. 1	+ 2 16	−3 7	137 56	July 5	− 1 7	+3 23	216 10
6	− 0 10	3 41	72 5	10	+ 1 8	3 54	149 59
11	2 35	4 13	6 15	15	3 23	4 23	83 50
16	4 58	4 44	300 25	20	5 37	4 52	17 40
21	7 17	5 12	234 35	25	7 46	5 17	311 31
26	9 32	5 37	168 45	30	9 51	5 41	245 23
31	11 40	6 1	102 55	Aug. 4	11 51	6 2	179 15
Feb. 5	13 41	6 20	37 5	9	13 46	6 21	113 8
10	15 36	6 37	331 15	14	15 34	6 37	47 3
15	17 22	6 52	265 25	19	17 16	6 51	340 58
20	19 0	7 2	199 34	24	18 49	7 1	274 53
25	20 29	7 10	133 43	29	20 16	7 9	208 49
Mar. 2	21 49	7 14	67 51	Sept. 3	21 34	7 14	142 47
7	22 59	7 15	1 58	8	22 44	7 15	76 45
12	23 59	7 13	296 5	13	23 45	7 13	10 44
17	24 50	7 7	230 11	18	24 37	7 8	304 43
22	25 29	6 58	164 16	23	25 19	7 1	238 43
27	25 59	6 46	98 20	28	25 52	6 49	172 44
Apr. 1	26 19	6 31	32 22	Oct. 3	26 14	6 35	106 46
6	26 27	6 13	326 23	8	26 25	6 18	40 47
11	26 24	5 52	260 24	13	26 26	5 58	334 50
16	26 10	5 29	194 23	18	26 16	5 35	268 52
21	25 46	5 3	128 21	23	25 53	5 10	202 56
26	25 10	4 35	62 17	28	25 20	4 43	136 59
May 1	24 23	4 6	356 13	Nov. 2	24 34	4 13	71 4
6	23 25	3 35	290 8	7	23 36	3 41	5 8
11	22 17	3 2	224 1	12	22 27	3 7	399 13
16	20 58	2 28	157 53	17	21 7	2 32	233 18
21	19 30	1 54	91 45	22	19 35	1 56	167 23
26	17 52	1 18	25 36	27	17 52	1 18	101 29
31	16 6	0 42	319 26	Dec. 2	15 59	+0 40	35 36
June 5	14 13	−0 6	253 16	7	13 58	−0 2	329 43
10	12 12	+0 30	187 5	12	11 49	0 37	263 50
15	10 6	1 6	120 54	17	9 34	1 15	197 57
20	7 55	1 41	54 43	22	7 13	1 52	132 5
25	5 41	2 16	348 32	27	4 49	2 29	66 13
30	− 3 25	+2 50	282 20	32	+ 2 23	−3 5	0 22

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic longitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node for 1907·0 to be 74° 27′·7. In computing L (the heliographic longitude of the centre of the Sun's disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

Note.—The times of minima only are given; the times of maxima may be found by adding the interv printed under the name of the star.

Y AURIGÆ*.

$22^{\mathrm{m}}\cdot0\ \ +42^{\circ}\ 21'\cdot6$
8·7 to 9·7
(Min. $7^{\mathrm{d}}\ 17^{\mathrm{h}}$.
Max. $0^{\mathrm{d}}\ 17\tfrac{1}{2}^{\mathrm{h}}$.)

an.		May		Sept.	
	h	d	h	d	h
	2	4	14	5	2
	13	20	1	20	12
	23				
eb.		June		Oct.	
	10	4	11	5	23
		19	22	21	9
ar.		July		Nov.	
	20	5	8	5	20
	7	20	18	21	6
pr.		Aug.		Dec.	
	17	5	5	6	17
	4	20	15	22	3

X² AURIGÆ.

$55^{\mathrm{m}}\cdot0\ \ +39^{\circ}\,49'\cdot0$
7·2 to 8·1
($4^{\mathrm{d}}\ 11^{\mathrm{h}}$.)

an.		May		Sept.	
	h	d	h	d	h
	2	2	9	11	10
	3	14	9	23	10
	4	26	9		
eb.		June		Oct.	
	5	7	10	5	10
	6	19	10	17	10
				29	9
ar.		July		Nov.	
	6	1	10	10	9
	7	13	10	22	9
	7	25	10		
pr.		Aug.		Dec.	
	8	6	10	4	8
	8	18	10	16	8
		30	10	28	7

T MONOCEROTIS.

$6^{\mathrm{h}}\ 20^{\mathrm{m}}\cdot2\ \ +7^{\circ}\ 8'\cdot2$
6·1 to 7·8
($7^{\mathrm{d}}\ 22\tfrac{1}{2}^{\mathrm{h}}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
18	7	6	9	18	10
Feb.		June		Oct.	
14	8	2	9	15	10
		29	9		
Mar.		July		Nov.	
13	8	26	9	11	11
Apr.		Aug.		Dec.	
9	8	22	10	8	11

W GEMINOR*.

$6^{\mathrm{h}}\ 29^{\mathrm{m}}\cdot6\ \ +15^{\circ}\ 24'\cdot2$
6·7 to 7·6
(Min. $7^{\mathrm{d}}\ 18^{\mathrm{h}}$.
Max. $2^{\mathrm{d}}\ 15\tfrac{1}{2}^{\mathrm{h}}$.)

Jan.		Apr.		Oct.	
d	h	d	h	d	h
1	16	22	12	5	18
17	12	May		21	14
Feb.		8	8	Nov.	
2	8	24	4	6	10
18	4	Aug.		22	6
Mar.		3	10	Dec.	
6	0	19	6	8	2
21	20	Sept.		23	22
Apr.		4	2		
6	16	19	22		

ζ GEMINOR.

$6^{\mathrm{h}}\ 58^{\mathrm{m}}\cdot6\ \ +20^{\circ}\ 42'\cdot4$
3·7 to 4·5
($5^{\mathrm{d}}\ 0\tfrac{1}{2}^{\mathrm{h}}$.)

Jan.		Mar.		May	
d	h	d	h	d	h
3	19	5	18	5	16
13	23	15	21	15	19
24	3	26	1	25	23
Feb.		Apr.		Aug.	
3	6	5	5	5	1
13	10	15	8	15	5
23	14	25	12	25	8

ζ GEMINOR

(*continued*).

Sept.		Oct.		Nov.	
d	h	d	h	d	h
4	12	15	3	24	18
14	16	25	6	Dec.	
24	19	Nov.		4	21
Oct.		4	10	15	1
4	23	14	14	25	5

X PUPPIS.

$7^{\mathrm{h}}\ 28^{\mathrm{m}}\cdot8\ \ -20^{\circ}\ 42'\cdot6$
8·0 to 9·0
($5^{\mathrm{d}}\ 4\tfrac{1}{2}^{\mathrm{h}}$.)

Jan.		May		Sept.	
d	d	d	h	d	h
5	5	14	23	22	17
31	4	June		Oct.	
Feb.		9	22	17	15
26	3	July		Nov.	
Mar.		5	20	12	14
24	1	31	19	Dec.	
Apr.		Aug.		8	13
19	0	26	18		

V CARINÆ*.

$8^{\mathrm{h}}\ 26^{\mathrm{m}}\cdot8\ \ -59^{\circ}\ 48'\cdot7$
7·4 to 8·1
(Min. $6^{\mathrm{d}}\ 16\tfrac{1}{2}^{\mathrm{h}}$.
Max. $2^{\mathrm{d}}\ 4^{\mathrm{h}}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
6	4	6	16	4	4
19	13	20	1	17	14
				30	23
Feb.		June		Oct.	
1	22	2	11	14	8
15	8	15	20	27	18
28	17	29	5		
Mar.		July		Nov.	
14	3	12	15	10	3
27	12	26	0	23	12
Apr.		Aug.		Dec.	
9	21	8	10	6	22
23	7	21	19	20	7

T VELORUM*.

$8^{\mathrm{h}}\ 34^{\mathrm{m}}\cdot7\ \ -47^{\circ}\ 2'\cdot1$
7·6 to 8·5
(Min. $4^{\mathrm{d}}\ 15^{\mathrm{h}}\cdot3$.
Max. $1^{\mathrm{d}}\ 9\tfrac{1}{2}^{\mathrm{h}}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	14	2	5	9	2
10	21	11	12	18	9
20	3	20	18	27	16
29	10	30	1		
Feb.		June		Oct.	
7	17	8	8	6	22
16	23	17	14	16	5
26	6	26	21	25	12
Mar.		July		Nov.	
7	13	6	4	3	19
16	20	15	10	13	1
26	2	24	17	22	8
Apr.		Aug.		Dec.	
4	9	3	0	1	15
13	16	12	7	10	21
22	22	21	13	20	4
		30	20	29	11

V VELORUM*.

$9^{\mathrm{h}}\ 19^{\mathrm{m}}\cdot5\ \ -55^{\circ}\ 33'\cdot7$
7·5 to 8·2
(Min. $4^{\mathrm{d}}\ 8^{\mathrm{h}}\cdot9$.
Max. $0^{\mathrm{d}}\ 23\tfrac{1}{2}^{\mathrm{h}}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	16	4	1	3	10
10	9	12	19	12	4
19	3	21	13	20	22
27	21	30	6	29	16
Feb.		June		Oct.	
5	15	8	0	8	12
14	9	16	18	17	3
23	2	25	12	25	21
Mar.		July		Nov.	
3	20	4	6	3	15
12	14	12	23	12	9
21	8	21	17	21	2
30	2	30	11	29	20
Apr.		Aug.		Dec.	
7	20	8	5	8	14
16	13	16	23	17	8
25	7	25	16	26	2

Y CARINÆ*

$10^{\mathrm{h}}\ 29^{\mathrm{m}}\cdot7\ \ -58^{\circ}$
8·1 to 8·6
(Min. $3^{\mathrm{d}}\ 15^{\mathrm{h}}\cdot4$
Max. $4^{\mathrm{d}}\ 17^{\mathrm{h}}$.)

Jan.		May		Se
d	h	d	h	d
3	8	7	2	7
10	15	14	9	15
17	21	21	16	22
25	4	28	22	29
Feb.		June		O
1	11	5	5	6
8	18	12	12	14
16	0	19	19	21
23	7	27	1	28
Mar.		July		N
2	14	4	8	5
9	20	11	15	12
17	3	18	21	19
24	10	26	4	26
31	17			
Apr.		Aug.		D
7	23	2	11	4
15	6	9	18	11
22	13	17	0	18
29	19	24	7	26
		31	14	

S MUSCÆ*.

$12^{\mathrm{h}}\ 7^{\mathrm{m}}\cdot8\ \ -69^{\circ}\ 3$
6·4 to 7·3
(Min. $9^{\mathrm{d}}\ 15\tfrac{1}{2}^{\mathrm{h}}$.
Max. $3^{\mathrm{d}}\ 11^{\mathrm{h}}$.)

Jan.		May		Se
d	h	d	h	d
8	20	4	17	16
28	4	24	1	
Feb.		June		O
16	11	12	8	6
				25
Mar.		July		N
7	19	1	16	13
27	2	21	0	
Apr.		Aug.		D
15	10	9	7	3
		28	15	22

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* *Alternate minima only* are given: the others can be readily found by adding the interval under he *name of the star.*

VARIABLES OF SHORT PERIOD (*continued*).

T CRUCIS*.

$12^{h}\,16^{m}{\cdot}3 \quad -61^{\circ}\,45'{\cdot}9$
$6{\cdot}8$ to $7{\cdot}6$
(Min. $6^{d}\,17^{h}{\cdot}6$.
Max. $2^{d}\,1\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	18	2	23	1	3
15	5	16	10	14	14
28	17	29	21	28	1
Feb.		June		Oct.	
11	4	12	8	11	12
24	15	25	19	24	23
Mar.		July		Nov.	
10	2	9	6	7	10
23	13	22	17	20	22
Apr.		Aug.		Dec.	
6	0	5	5	4	9
19	11	18	16	17	20
				31	7

R CRUCIS*.

$12^{h}\,18^{m}{\cdot}5 \quad -61^{\circ}\,6'{\cdot}8$
$6{\cdot}8$ to $8{\cdot}0$
(Min. $5^{d}\,19^{h}{\cdot}8$.
Max. $1^{d}\,9\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
1	7	9	10	2	22
12	22	21	2	14	14
24	14			26	5
Feb.		June		Oct.	
5	6	1	17	7	21
16	21	13	9	19	13
28	13	25	1	31	4
Mar.		July		Nov.	
12	4	6	16	11	20
23	20	18	8	23	11
		29	23		
Apr.		Aug.		Dec.	
4	11	10	15	5	3
16	3	22	7	16	19
27	19			28	10

S CRUCIS*.

$12^{h}\,48^{m}{\cdot}9 \quad -57^{\circ}\,55'{\cdot}5$
$6{\cdot}5$ to $7{\cdot}6$
(Min. $4^{d}\,16^{h}{\cdot}6$.
Max. $1^{d}\,12^{h}$.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
0	18	7	6	7	9
10	3	16	15	16	19
19	12	26	0	26	4
28	21				

S CRUCIS*

(*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
4	13	7	8	9	3
13	22	16	17	18	12
23	7	26	2	27	21
May		Aug.		Nov.	
2	16	4	11	6	7
12	1	13	20	15	16
21	10	23	6	25	1
30	20				
June		Sept.		Dec.	
9	5	1	15	4	10
18	14	11	0	13	19
27	23	20	9	23	4
		29	18		

W VIRGINIS.

$13^{h}\,21^{m}{\cdot}2 \quad -2^{\circ}\,53'{\cdot}8$
$8{\cdot}9$ to $10{\cdot}1$
($8^{d}\,4\frac{1}{2}^{h}$.)

Jan.		Apr.		Aug.	
d	h	d	h	d	h
11	18	25	9	7	0
29	1			24	7
		May			
Feb.		12	16	Nov.	
15	7	29	22	1	9
Mar.		June		18	15
4	14	16	5		
21	20			Dec.	
		July		5	22
Apr.		3	11	23	4
8	3	20	18		

V CENTAURI*.

$14^{h}\,25^{m}{\cdot}9 \quad -56^{\circ}\,28'{\cdot}5$
$6{\cdot}5$ to $7{\cdot}8$
(Min. $5^{d}\,11^{h}{\cdot}9$.
Max. $1^{d}\,11\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	18	3	15	1	11
13	17	14	14	12	11
24	17	25	14	23	11
Feb.		June		Oct.	
4	17	5	14	4	10
15	17	16	13	15	10
26	16	27	13	26	10
Mar.		July		Nov.	
9	16	8	13	6	10
20	16	19	12	17	9
31	15	30	12	28	9
Apr.		Aug.		Dec.	
11	15	10	12	9	1
22	15	21	12	20	8
				31	8

R TRIANG. AUSTR.*

$15^{h}\,11^{m}{\cdot}4 \quad -66^{\circ}\,9'{\cdot}3$
$6{\cdot}7$ to $7{\cdot}4$
(Min. $3^{d}\,9^{h}{\cdot}3$.
Max. $4^{d}\,9\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
3	17	5	17	4	17
10	11	12	12	11	12
17	6	19	6	18	7
24	1	26	1	25	1
30	19				
Feb.		June		Oct.	
		1	20	1	20
6	14	8	14	8	15
13	9	15	9	15	9
20	3	22	4	22	4
26	22	28	22	28	23
Mar.		July		Nov.	
5	17	5	17	4	17
12	11	12	12	11	12
19	6	19	6	18	7
26	1	26	1	25	1
Apr.		Aug.		Dec.	
1	20	1	20	1	20
8	14	8	15	8	15
15	9	15	9	15	9
22	4	22	4	22	4
28	22	28	23	28	23

U NORMÆ.

$15^{h}\,35^{m}{\cdot}2 \quad -55^{\circ}\,0'{\cdot}7$
$8{\cdot}7$ to $9{\cdot}8$
($6^{d}\,0^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
3	3	9	16	13	5
15	19	22	8	25	21
28	10				
Feb.		June		Oct.	
10	2	3	23	8	13
22	18	16	15	21	4
		29	7		
Mar.		July		Nov.	
7	9	11	23	2	20
20	1	24	14	15	12
				28	4
Apr.		Aug.		Dec.	
1	17	6	6	10	19
14	9	18	22	23	11
27	0	31	13		

S TRIANG. AUSTR.*

$15^{h}\,52^{m}{\cdot}8 \quad -63^{\circ}\,30'{\cdot}7$
$6{\cdot}4$ to $7{\cdot}4$
(Min. $6^{d}\,7^{h}{\cdot}8$.
Max. $2^{d}\,2\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
6	4	12	15	3	11
18	20	25	7	16	2
31	11			28	18
Feb.		June		Oct.	
13	2	6	22	11	9
25	18	19	14	24	1
Mar.		July		Nov.	
10	10	2	5	5	16
23	1	14	21	18	8
		27	12	30	23
Apr.		Aug.		Dec.	
4	17	9	4	13	15
17	8	21	19	26	6
30	0				

S NORMÆ*.

$16^{h}\,11^{m}{\cdot}2 \quad -57^{\circ}\,40'{\cdot}2$
$6{\cdot}6$ to $7{\cdot}5$
(Min. $9^{d}\,18^{h}$.
Max. $4^{d}\,9\frac{1}{2}^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
4	5	1	6	14	19
23	17	20	18		
Feb.		June		Oct.	
12	5	9	6	4	7
		28	18	23	19
Mar.		July		Nov.	
3	17	18	6	12	7
23	6				
Apr.		Aug.		Dec.	
11	18	6	18	1	19
		26	7	21	7

V² SCORPII*.

$16^{h}\,52^{m}{\cdot}3 \quad -33^{\circ}\,27'{\cdot}9$
$6{\cdot}8$ to $7{\cdot}9$
(Min. $6^{d}\,1^{h}{\cdot}5$.
Max. $1^{d}\,10^{h}$.)

Jan.		Mar.		May	
d	h	d	h	d	h
0	20	2	11	2	2
12	23	14	14	14	5
25	2	26	17	26	8
Feb.		Apr.		June	
6	5	7	20	7	11
18	8	19	23	19	14

V² Sco

(*conti*

July		Au
d	h	d
1	17	19
13	20	31
25	22	
		Se
Aug.		12
7	1	24

X SAGIT

$17^{h}\,41^{m}{\cdot}7$ -
$4{\cdot}0$ to
(Min.
Max. 2

Feb.		Ma
d	h	d
4	10	27
18	11	
		Ju
Mar.		10
4	11	24
18	12	
Apr.		Ju
1	12	8
15	15	22
29	14	
		Au
May		5
13	14	19

Y OPH

$17^{h}\,47^{m}{\cdot}7$
$6{\cdot}2$ to
(6^{d} 5

Jan.		M
d	h	d
14	19	14
31	22	31
Feb.		Ju
18	1	17
Mar.		Ju
7	4	5
24	7	22
Apr.		Au
10	10	
27	13	9

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* Alternate *minima only are given*: the others can be readily found by adding the int
the name of the star.

VARIABLES OF SHORT PERIOD (*continued*).

SAGITTARII *.

$^{h}59^{m}{\cdot}1\ -29^{\circ}\ 35'{\cdot}2$
4·8 to 5·8
(Min. $7^{d}\ 14^{h}{\cdot}3$.
Max. $3^{d}\ 0^{h}$.)

eb.		May		Aug.	
	h	d	h	d	h
	9	20	16	19	20
	13				
				Sept.	
ar.		June		4	0
	18	4	21	19	5
	22	20	1		
				Oct.	
pr.		July		4	9
	3	5	6	19	14
	7	20	11		
				Nov.	
ay		Aug.		3	18
	12	4	15	18	23

SAGITTARII *.

$^{h}15^{m}{\cdot}9\ -18^{\circ}\ 54'{\cdot}2$
5·8 to 6·6
(Min. $5^{d}\ 18^{h}{\cdot}6$.
Max. $2^{d}\ 2\frac{1}{2}^{h}$.)

eb.		May		Aug.	
	h	d	h	d	h
	4	28	15	17	11
	17			29	0
	6				
		June		Sept.	
ar.					
	19	9	4	9	13
	8	20	17	21	2
	21				
		July		Oct.	
pr.		2	6	2	15
	10	13	19	14	4
	23	25	8	25	17
ay				Nov.	
	12	Aug.		6	6
	2	5	21	17	20

SAGITTARII *.

$^{h}26^{m}{\cdot}4\ -19^{\circ}\ 11'{\cdot}6$
7·0 to 8·3
(Min. $6^{d}\ 17^{h}{\cdot}9$.
Max. $2^{d}\ 23\frac{1}{2}^{h}$.)

eb.		Mar.		Mar.	
	h	d	h	d	h
	20	1	19	28	19
	8	15	7		

U SAGITTARII* (*continued*).

Apr.		June		Sept.	
d	h	d	h	d	h
11	7	17	17	6	16
24	18			20	4
		July			
May		1	5	Oct.	
8	6	14	17	3	15
21	18	28	5	17	3
				30	15
		Aug.			
June		10	16	Nov.	
4	6	24	4	13	3

β LYRÆ.

$18^{h}\ 46^{m}{\cdot}7\ +33^{\circ}\ 15'{\cdot}2$
3·4 to 4·5
(Max. $3^{d}\ 2^{h}$.
Secondary Min.
$6^{d}\ 11^{h}$.
Max. $9^{d}\ 18^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
10	11	6	18	12	23
23	10	19	16	25	21
Feb.		June		Oct.	
5	8	1	14	8	19
18	6	14	12	21	17
		27	10		
Mar.		July		Nov.	
3	4			3	15
16	2	10	8	16	13
29	0	23	6	29	11
Apr.		Aug.		Dec.	
10	22	5	4	12	9
23	20	18	2	25	10
		31	1		

κ PAVONIS*.

$18^{h}\ 47^{m}{\cdot}4\ -67^{\circ}\ 21'{\cdot}1$
3·8 to 5·2
(Min. $9^{d}\ 2\frac{1}{2}^{h}$.
Max. $4^{d}\ 9^{h}$.)

Jan.		Mar.		May	
d	h	d	h	d	h
3	20	17	15	11	6
22	1			29	10
Feb.		Apr.		June	
9	6	4	20	16	15
27	10	23	1		

κ PAVONIS* (*continued*).

July		Sept.		Nov.	
d	h	d	h	d	h
4	20	15	15	9	5
23	1			27	10
Aug.		Oct.		Dec.	
10	6	3	20	15	15
28	10	22	1		

U AQUILÆ *.

$19^{h}\ 24^{m}{\cdot}4\ -7^{\circ}\ 14'{\cdot}1$
6·4 to 7·1
(Min. $7^{d}\ 0^{h}{\cdot}6$.
Max. $2^{d}\ 4\frac{1}{2}^{h}$.)

Feb.		May		Sept.	
d	h	d	h	d	h
6	8	29	17	5	1
20	9			19	2
Mar.		June		Oct.	
6	10	12	18	3	3
20	11	26	19	17	5
				31	6
Apr.		July		Nov.	
3	12	10	20	14	7
17	14	24	22	28	8
May		Aug.			
1	15	7	23		
15	16	22	0		

U VULPECULÆ *.

$19^{h}\ 32^{m}{\cdot}6\ +20^{\circ}\ 7'{\cdot}5$
6·9 to 7·6
(Min. $7^{d}\ 23\frac{1}{2}^{h}$.
Max. $2^{d}\ 3^{h}$.)

Jan.		Apr.		July	
d	h	d	h	d	h
5	17	11	11	16	5
21	16	27	10		
				Aug.	
Feb.		May		1	4
6	15	13	9	17	3
22	14	29	8		
				Sept.	
Mar.		June		2	2
10	13	14	7	18	1
26	12	30	6		

U VULPECULÆ* (*continued*).

Oct.		Nov.		Dec.	
d	h	d	h	d	h
4	0	4	22	6	20
19	23	20	21	22	20

T^{3} CYGNI*.

$19^{h}\ 41^{m}{\cdot}1\ +29^{\circ}\ 2'{\cdot}4$
6·6 to 7·4
(Min. $3^{d}\ 20^{h}{\cdot}3$.
Max. $1^{d}\ 8^{h}$.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	3	5	5	5	6
9	20	12	22	12	23
17	12	20	14	20	16
25	5	28	7	28	9
Feb.		June		Oct.	
1	21	4	23	6	1
9	14	12	16	13	18
17	7	20	8	21	10
24	23	28	1	29	3
Mar.		July		Nov.	
4	16	5	17	5	19
12	9	13	10	13	12
20	1	21	3	21	4
27	18	28	20	28	21
Apr.		Aug.		Dec.	
3	10	5	12	6	14
11	3	13	5	14	7
19	19	20	21	21	23
27	12	28	14	29	16

η AQUILÆ *.

$19^{h}\ 47^{m}{\cdot}7\ +0^{\circ}\ 46'{\cdot}0$
3·5 to 4·7
(Min. $7^{d}\ 4^{h}{\cdot}2$.
Max. $2^{d}\ 9^{h}$.)

Feb.		June		Oct.	
d	h	d	h	d	h
2	13	11	17	4	13
16	21	26	1	18	21
Mar.		July		Nov.	
3	6	10	10	2	6
17	14	24	18	16	14
31	23			30	23
Apr.		Aug.			
15	7	8	3		
29	16	22	11	Dec.	
May		Sept.		15	7
14	0	5	20	29	15
28	8	20	4		

S SAGITTÆ *

$19^{h}\ 51^{m}{\cdot}8\ +16^{\circ}\ 2$
5·6 to 6·4
(Min. $8^{d}\ 9^{h}{\cdot}2$.
Max. $2^{d}\ 23^{h}$.)

Feb.		May		Se
d	h	d	h	d
6	19	18	9	12
23	14			29
		June		O
Mar.		4	4	
12	8	20	22	16
29	3	July		N
Apr.		7	16	2
14	21	24	11	18
		Aug.		D
May		10	5	5
1	15	26	23	22

X CYGNI.

$20^{h}\ 39^{m}{\cdot}8\ +35^{\circ}\ 1$
6·4 to 7·5
($6^{d}\ 5^{h}$.)

Jan.		May		Se
d	h	d	h	d
14	13	9	6	17
30	22	25	15	
				O
Feb.		June		3
16	8	11	0	20
		27	10	
Mar.				N
		July		
4	17			5
21	2	13	19	21
		30	4	
Apr.				D
		Aug.		
6	11			8
22	21	15	13	24
		31	23	

T VULPECULÆ

$20^{h}\ 47^{m}{\cdot}5\ +27^{\circ}\ 5$
5·5 to 6·5
(Min. $4^{d}\ 10\frac{1}{2}^{h}$.
Max. $1^{d}\ 9\frac{1}{2}^{h}$.)

Jan.		Feb.		M
d	h	d	h	d
3	4	7	16	6
12	1	16	13	15
20	22	25	10	24
29	19			

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* *Alternate minima only are given:* the others can be readily found by adding the interval under name of the star.

VARIABLES OF SHORT PERIOD (*continued*).

T VULPECULÆ *

(*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
1	21	8	11	5	5
10	18	17	8	14	2
19	15	26	5	22	22
28	12			31	19
May		Aug.		Nov.	
7	9	4	2	9	16
16	6	12	23	18	13
25	3	21	20	27	10
		30	17		
June		Sept.		Dec.	
3	0	8	14	6	7
11	21	17	11	15	4
20	18	26	8	24	1
29	15				

W⁵ CYGNI.

$20^h 53^m\cdot 8$ $+39^\circ 49'\cdot 1$
9·0 to 10·0
($6^d\ 4^h$.)

Jan.		Mar.		May	
d	h	d	h	d	h
9	7	10	16	10	1
29	10	30	19	30	4
Feb.		Apr.		June	
18	13	19	22	19	7

W⁵ CYGNI

(*continued*).

July		Sept.		Nov.	
d	h	d	h	h	d
9	10	7	19	7	4
29	13	27	22	27	7
Aug.		Oct.		Dec.	
18	16	18	1	17	10

U⁴ CYGNI.

$20^h 56^m\cdot 7$ $+42^\circ 14'\cdot 1$
8·5 to 10·0
($5^d\ 3^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
7	15	5	12	15	2
22	9	20	5	29	20
Feb.		June		Oct.	
6	2	3	23	14	13
20	20	18	17	29	7
Mar.		July		Nov.	
7	13	3	10	13	1
22	7	18	4	27	18
Apr.		Aug.		Dec.	
6	1	1	21	12	12
20	18	16	15	27	5
		31	9		

X⁵ CYGNI *.

$21^h\ 0\cdot 7$ $+39^\circ 36'\cdot 0$
8·8 to 9·5
(Min. $7^d\ 20^h\cdot 6$.
Max. $2^d\ 2\frac{1}{2}^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
2	18	8	11	11	5
18	12	24	5	26	22
Feb.		June		Oct.	
3	5	8	22	12	15
18	22	24	15	28	8
Mar.		July		Nov.	
6	15	10	8	13	1
22	8	26	1	28	18
Apr.		Aug.		Dec.	
7	1	10	18	14	11
22	18	26	11	30	4

δ CEPHEI *.

$22^h 25^m\cdot 7$ $+57^\circ 56'\cdot 3$
3·7 to 4·9
(Min. $5^d\ 8^h\cdot 8$.
Max. $1^d\ 9\frac{1}{2}^h$.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
5	11	6	15	10	20
16	4	17	9	21	13
26	22	28	2		

δ CEPHEI *

(*continued*).

Apr.		July		Oct.	
d	h	d	h	d	h
1	7	6	21	11	11
12	1	17	15	22	5
22	18	28	8		
May		Aug.		Nov.	
3	12	8	2	1	22
14	5	18	19	12	16
24	23	29	13	23	10
June		Sept.		Dec.	
4	16	9	7	4	3
15	10	20	0	14	21
26	4	30	18	25	14

S² CASSIOPEIÆ *.

$23^h 32^{\circ}\cdot 9$ $+61^\circ 54'\ 9$
9·0 to 11·0
(Min. $6^d\ 7^h\cdot 2$.
Max $1^d\ 19\frac{1}{2}^h$.)

Jan.		Mar.		May	
d	h	d	h	d	h
0	6	4	5	6	5
12	20	16	20	18	19
25	11	29	10	31	9
Feb.		Apr.		June	
7	1	11	0	12	23
19	15	23	14	25	14

S² CASSIO

(*continu*

July		Sept	
d	h	d	
8	4	9	
20	18	21	1
Aug.		Oct.	
2	9	4	
14	23	16	2
27	13	29	1

Y² CASSIO

$23^h 47^m\cdot 5+5$
9·1 to 1
($4^d\ 17$

Jan.		May	
d	h	d	
4	10	5	2
16	14	18	
28	17	30	
Feb.		June	
9	21	11	
22	0	23	1
Mar.		July	
6	[illegible]	5	1
18	7	17	1
30	10	29	2
Apr.		Aug	
11	14	11	
23	17	23	

MAXIMA OF U PEGASI.

$23^h 53^m\cdot 2$ $+15^\circ 26'\cdot 2$ 9·2 to 10·

Jan.	Feb.	April.	May.	June.	July.
d h m	d h m	d h m	d h m	d h m	d h m
3 14 42	2 14 17	3 13 27	3 13 2	2 12 38	2 12 13
7 8 39	6 8 14	7 12 24	7 6 59	6 16 34	6 6 7
11 2 36	10 2 11	11 1 21	11 0 56	10 0 31	10 0 6
14 20 33	13 20 8	14 19 18	14 18 53	13 18 28	13 18 3
18 14 30	17 14 5	18 13 15	18 12 50	17 12 25	17 12 0
22 8 26	21 8 2	22 7 12	22 6 47	21 6 22	21 5 57
26 2 23	25 1 58	26 1 9	26 0 44	25 0 19	24 23 54
29 20 20		29 19 6	29 18 41	28 18 16	28 17 51
August.	Sept.	Oct.	Nov.	Dec.	
1 11 48	4 5 20	4 4 55	3 4 30	3 4 5	
5 5 45	7 23 17	7 22 52	6 22 27	6 22 2	
8 23 42	11 17 14	11 16 49	10 16 24	10 15 59	
12 17 38	15 11 10	15 10 46	14 10 21	14 9 56	
16 11 35	19 5 7	19 4 42	18 4 18	18 3 53	
20 5 32	22 23 4	22 22 39	21 22 15	21 21 50	
23 23 29	26 17 1	26 16 36	25 16 11	25 15 47	
27 17 26	30 10 58	30 10 33	29 10 8	29 9 43	
31 11 23					

Every twentieth maximum is given; the others may be found by adding *multiples of the period* $4^h\ 29^m\cdot 8$. The minimum takes place $2^h\ 15^m$ after the *maximum.*

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

U Cephei.	Z Persei.	Z² Cassiop.	Algol.	T² Persei.	λ Tauri.	W² Tauri.
$0^{h}\ 54^{m}{\cdot}0$	$2^{h}\ 34^{m}{\cdot}1$	$2^{h}\ 40^{m}{\cdot}5$	$3^{h}\ 2^{m}{\cdot}1$	$3^{h}\ 17^{m}{\cdot}2$	$3^{h}\ 55^{m}{\cdot}5$	$3^{h}\ 58^{m}{\cdot}2$
$+81^{\circ}\ 22'{\cdot}5$	$+41^{\circ}\ 47'{\cdot}9$	$+69^{\circ}\ 14'{\cdot}6$	$+40^{\circ}\ 35'{\cdot}9$	$+46^{\circ}\ 13'{\cdot}7$	$+12^{\circ}\ 13'{\cdot}7$	$+27^{\circ}\ 52'{\cdot}2$
7·1 to 9·2	9·1 to 12·5	6·4 to 7·7	2·3 to 3·5	9·5 to 11·0	3·4 to 4·2	7·2 to 11·0
Fourth.	*Fourth.*	*Tenth.*	*Third.*	*Twelfth.*	*Second.*	*Fourth.*
$2^{d}\ 11^{h}\ 49^{m}{\cdot}7$.	$3^{d}\ 1^{h}\ 21^{m}{\cdot}4$.	$1^{d}\ 4^{h}\ 41^{m}{\cdot}0$.	$2^{d}\ 20^{h}\ 49^{m}$.	$0^{d}\ 20^{h}\ 23^{m}{\cdot}2$.	$3^{d}\ 22^{h}\ 52^{m}$.	$2^{d}\ 18^{h}\ 27^{m}{\cdot}2$.
Jan. d h m 1 15 25 11 14 44 21 14 3 31 13 22 Feb. 10 12 41 20 12 0 March. 2 11 19 12 10 37 22 9 56 April. 1 9 15 11 8 34 21 7 53 May. 1 7 12 11 6 30 21 5 49 31 5 8 June. 10 4 27 20 3 46 30 3 5 July. 10 2 24 20 1 43 30 1 2 August. 9 0 21 18 23 40 28 22 59 Sept. 7 22 18 17 21 37 27 20 56 Oct. 7 20 15 17 19 34 27 18 53 Nov. 6 18 12 16 17 31 26 16 50 Dec. 6 16 9 16 15 28 26 14 47	Jan. d h m 1 16 13 13 21 39 26 3 4 Feb. 7 8 30 19 13 56 March. 3 19 21 16 0 47 28 6 12 April. 9 11 38 21 17 4 May. 3 22 29 16 3 55 28 9 20 June. 9 14 46 21 20 12 July. 4 1 37 16 7 3 28 12 28 August. 9 17 54 21 23 20 Sept. 3 4 45 15 10 11 27 15 36 Oct. 9 21 2 22 2 28 Nov. 3 7 53 15 13 19 27 18 44 Dec. 10 0 10 22 5 36	Jan. d h m 0 12 40 12 11 30 24 10 20 Feb. 5 9 10 17 8 0 March. 1 6 50 13 5 40 25 4 30 April. 6 3 20 18 2 10 30 1 0 May. 11 23 50 23 22 40 June. 4 21 30 16 20 20 28 19 10 July. 10 18 0 22 16 50 August. 3 15 40 15 14 30 27 13 20 Sept. 8 12 10 20 11 0 Oct. 2 9 50 14 8 40 26 7 30 Nov. 7 6 20 19 5 10 Dec. 1 4 0 13 2 50 25 1 40	Jan. d h m 3 3 26 11 17 53 20 8 20 28 22 47 Feb. 6 13 14 15 3 41 23 18 8 March. 4 8 34 12 23 1 21 13 28 30 3 55 April. 7 18 22 16 8 49 24 23 16 June. 12 17 8 21 7 35 29 22 2 July. 8 12 29 17 2 56 25 17 23 August. 3 7 50 11 22 16 20 12 43 29 3 10 Sept. 6 17 37 15 8 4 23 22 31 Oct. 2 12 58 11 3 25 19 17 52 28 8 18 Nov. 5 22 45 14 13 12 23 3 39 Dec. 1 18 6 10 8 33 18 23 0 27 13 27	Jan. d h m 0 5 56 10 10 35 20 15 13 30 19 52 Feb. 10 0 30 20 5 8 March. 2 9 47 12 14 25 22 19 4 April. 1 23 42 12 4 20 22 8 59 May. 2 13 37 12 18 16 22 22 54 June. 2 3 32 12 8 11 22 12 49 July. 2 17 28 12 22 6 23 2 44 August. 2 7 23 12 12 1 22 16 40 Sept. 1 21 18 12 1 56 22 6 35 Oct. 2 11 13 12 15 52 22 20 30 Nov. 2 1 8 12 5 47 22 10 25 Dec. 2 15 4 12 19 42 23 0 20	Jan. d h m 1 12 54 9 10 38 17 8 23 25 6 7 Feb. 2 3 51 10 1 36 17 23 20 25 21 5 March. 5 18 49 13 16 33 21 14 18 29 12 2 April. 6 9 47 14 7 31 June. 24 11 11 July. 2 8 55 10 6 39 18 4 24 26 2 8 August. 2 23 53 10 21 37 18 19 21 26 17 6 Sept. 3 14 50 11 12 35 19 10 19 27 8 3 Oct. 5 5 48 13 3 32 21 1 17 28 23 1 Nov. 5 20 45 13 18 30 21 16 14 29 13 59 Dec. 7 11 43 15 9 27 23 7 12 31 4 56	Jan. d h m 1 17 2 12 18 50 23 20 39 Feb. 3 22 28 15 0 16 26 2 5 March. 9 3 54 20 5 42 31 7 31 April. 11 9 19 22 11 8 July. 3 10 54 14 12 43 25 14 31 August. 5 16 20 16 18 9 27 19 57 Sept. 7 21 46 18 23 35 30 1 23 Oct. 11 3 12 22 5 1 Nov. 2 6 49 13 8 38 24 10 26 Dec. 5 12 15 16 14 4 27 15 52

Minima of Variable Stars of the Algol Type (continued).

V² PERSEI.
$4^h\ 4^{m}\cdot6$
$+34^\circ\ 0'\cdot5$
9·3 to 11·0
Fourth.
$1^d\ 23^h\ 22^{m}\cdot1$.

d	h	m
Jan.		
7	13	46
15	11	14
23	8	42
31	6	11
Feb.		
8	3	39
16	1	8
23	22	36
March.		
3	20	4
11	17	33
19	15	1
27	12	30
April.		
4	9	58
12	7	26
20	4	55
28	2	23
May.		
5	23	52
13	21	20
21	18	48
29	16	17
June.		
6	13	45
14	11	14
22	8	42
30	6	10
July.		
8	3	39
16	1	7
23	22	36
31	20	4
August.		
8	17	32
16	15	1
24	12	29
Sept.		
1	9	58
9	7	26
17	4	54
25	2	23
Oct.		
2	23	51
10	21	20
18	18	48
26	16	16
Nov.		
3	13	45
11	11	13
19	8	42
27	6	10
Dec.		
5	3	38
13	1	7
20	22	35
28	20	4

W² PERSEI.
$4^h\ 13^{m}\cdot8$
$+42^\circ\ 5'\cdot4$
9·5 to 11·5
$13^d\ 4^h\ 46\frac{1}{2}^{m}$.

d	h	m
Jan.		
13	12	8
26	16	54
Feb.		
8	21	41
22	2	27
March.		
7	7	14
20	12	0
April.		
2	16	47
15	21	33
29	2	20
May.		
12	7	7
25	11	53
June.		
7	16	40
20	21	26
July.		
4	2	13
17	6	59
30	11	46
August.		
12	16	33
25	21	19
Sept.		
8	2	6
21	6	52
Oct.		
4	11	39
17	16	25
30	21	12
Nov.		
13	1	58
26	6	45
Dec.		
9	11	32
22	16	18

S² CEPHEI.
$4^h\ 49^{m}\cdot8$
$+80^\circ\ 6'\cdot5$
9·5 to 12·0
$12^d\ 10^h\ 5^m$.

d	h	m
Jan.		
11	14	24
24	0	29
Feb.		
5	10	34
17	20	39
March.		
2	6	43
14	16	48
27	2	53
April.		
8	12	58
20	23	3
May.		
3	9	7
15	19	12
28	5	17
June.		
9	15	22
22	1	27
July.		
4	11	31
16	21	36
29	7	41
August.		
10	17	46
23	3	51
Sept.		
4	13	55
17	0	0
29	10	5
Oct.		
11	20	10
24	6	15
Nov.		
5	16	19
18	2	24
30	12	29
Dec.		
12	22	34
25	8	39

W² GEMINOR.
$5^h\ 55^{m}\cdot8$
$+23^\circ\ 8'\cdot3$
9·5 to 11·0
Fifth.
$2^d\ 20^h\ 46^{m}\cdot9$

d	h	m
Jan.		
1	17	30
16	1	24
30	9	18
Feb.		
13	17	13
28	1	7
March.		
14	9	2
28	16	56
April.		
12	0	50
26	8	45
May.		
10	16	39
25	0	33
Aug.		
4	16	6
19	0	0
Sept.		
2	7	54
16	15	49
30	23	43
Oct.		
15	7	38
29	15	32
Nov.		
12	23	26
27	7	21
Dec.		
11	15	15
25	23	10

U² MONOCER.
$6^h\ 49^{m}\cdot7$
$-7^\circ\ 28'\cdot7$
9·8 to 10·5
Tenth.
$0^d\ 21^h\ 30^{m}\cdot5$

d	h	m
Jan.		
0	0	20
8	23	24
17	22	29
26	21	34
Feb.		
4	20	38
13	19	43
22	18	48
March.		
3	17	52
12	16	57
21	16	2
30	15	6
April.		
8	14	11
17	13	16
26	12	20
May.		
5	11	25
14	10	30
Aug.		
3	2	12
12	1	16
21	0	21
29	23	26
Sept.		
7	22	30
16	21	35
25	20	40
Oct.		
5	19	44
13	18	49
22	17	54
31	16	58
Nov.		
9	16	3
18	15	8
27	14	12
Dec.		
6	13	17
15	12	22
24	11	26

R CANIS MIN.
$7^h\ 3^{m}\cdot6$
$+10^\circ\ 10'\cdot2$
7·5 to 9·8
Eighth.
$1^d\ 3^h\ 15^{m}\cdot8$.

d	h	m
Jan.		
0	14	16
9	16	22
18	18	28
27	20	34
Feb.		
5	22	40
15	0	46
24	2	52
March.		
5	4	58
14	7	5
23	9	11
April.		
1	11	17
10	13	23
19	15	29
28	17	35
May.		
7	19	41
16	21	48
25	23	54
Sept.		
2	23	1
12	1	7
21	3	13
30	5	20
Oct.		
9	7	26
18	9	32
27	11	38
Nov.		
5	13	44
14	15	50
23	17	56
Dec.		
2	20	2
12	22	9
21	0	15
30	2	21

Y CAMELOP.
$7^h\ 28^{m}\cdot6$
$76^\circ\ 16'\cdot0$
9·5 to 12·0
Second.
$3^d\ 7^h\ 20^{m}\cdot0$.

d	h	m
Jan.		
6	10	38
13	1	18
19	15	58
26	6	38
Feb.		
1	21	18
8	11	58
15	2	38
21	17	18
28	7	58
March.		
6	22	38
13	13	18
20	3	58
26	18	38
April.		
2	9	18
8	23	58
15	14	38
22	5	18
28	19	58
May.		
5	10	38
12	1	18
18	15	58
25	6	38
31	21	18
June.		
7	11	58
14	2	38
20	17	18
27	7	58
July.		
3	22	38
10	13	18
17	3	58
23	18	38
30	9	18
August.		
5	23	58
12	14	39
19	5	19
25	19	59
Sept.		
1	10	39
8	1	19
14	15	59
21	6	39
27	21	19
Oct.		
4	11	59
11	2	39
17	17	19
24	7	5
30	22	

Minima of Variable Stars of the Algol Type (continued).

Y Camelop.
(*con.*).
Nov.
d h m
6 13 19
13 3 59
19 18 39
26 9 19
Dec.
2 23 59
9 14 39
16 5 19
22 19 59
29 10 39

R^2 Puppis.
$7^{h}\,43^{m}{\cdot}8$
$-41^{\circ}\,8'{\cdot}9$
9·8 to 11·0
Second.
$6^{d}\,10^{h}\,19^{m}{\cdot}5$.
Jan.
d h m
4 23 57
17 20 36
30 17 14
Feb.
12 13 53
25 10 31
March.
10 7 9
23 3 48
April.
5 0 26
17 21 5
30 17 43
May.
13 14 21
26 11 0
June.
8 7 38
21 4 17
July.
4 0 55
16 21 33
29 18 12
August.
11 14 50
24 11 29
Sept.
6 8 7
19 4 45
Oct.
2 1 24
14 22 2
27 18 41
Nov.
9 15 19
22 11 57
Dec.
5 8 36
8 5 14
1 53

V Puppis.
$7^{h}\,55^{m}{\cdot}6$
$-48^{\circ}\,59'{\cdot}5$
4·1 to 4·8
Eighth.
$1^{d}\,10^{h}\,54^{m}{\cdot}4$.
Jan.
d h m
0 17 25
12 8 41
23 23 56
Feb.
4 15 12
16 6 27
27 21 43
March.
11 12 59
23 4 14
April.
3 19 30
15 10 45
27 2 1
May.
8 17 16
20 8 32
31 23 47
June.
12 15 3
24 6 19
July.
5 21 34
17 12 50
29 4 5
August.
9 19 21
21 10 36
Sept.
2 1 52
13 17 8
25 8 23
Oct.
6 23 39
18 14 54
30 6 10
Nov.
10 21 25
22 12 41
Dec.
4 3 56
15 19 12
27 10 28

S Cancri.
$8^{h}\,38^{m}{\cdot}6$
$+19^{\circ}\,22'{\cdot}2$
8·2 to 9·8
Jan.
d h m
3 16 15
13 3 52
22 15 30
Feb.
1 3 8
10 14 46
20 2 23
March.
1 14 1
11 1 39
20 13 17
30 0 54
April.
8 12 32
18 0 10
27 11 48
May.
6 23 25
16 11 3
25 22 41
June.
4 10 19
13 21 56
23 9 34
Sept.
7 6 36
16 18 14
26 5 52
Oct.
5 17 29
15 5 7
24 16 45
Nov.
3 4 23
12 16 0
22 3 38
Dec.
1 15 16
11 2 54
20 14 31
30 2 9

S Antliæ.
$9^{h}\,28^{m}{\cdot}2$
$-28^{\circ}\,13'{\cdot}1$
6·7 to 7·3
Fortieth.
$0^{d}\,7^{h}\,46^{m}{\cdot}8$.
Jan.
d h m
6 11 18
19 10 30
Feb.
1 9 43
14 8 55
27 8 7
March.
12 7 19
25 6 31
April.
7 5 43
20 4 55
May.
3 4 8
16 3 20
29 2 32
June.
11 1 44
24 1 56
July.
7 0 8
19 23 21
Sept.
3 8 33
16 7 45
29 6 57
Oct.
12 6 10
25 5 22
Nov.
7 4 34
20 3 46
Dec.
3 2 58
16 2 10
29 1 23

β Lyræ type.

S Velorum.
$9^{h}\,29^{m}{\cdot}7$
$-44^{\circ}\,47'{\cdot}8$
8·2 to 8·6
Second.
$5^{d}\,22^{h}\,24^{m}{\cdot}4$.
Jan.
d h m
2 12 40
14 9 29
26 6 17
Feb.
7 3 6
18 23 55
March.
2 20 43
14 17 32
26 14 21
April.
7 11 10
19 7 58
May.
1 4 47
13 1 36
24 22 24
June.
5 19 13
17 16 2
29 12 50
July.
11 9 39
23 6 28
August.
4 3 17
16 0 5
27 20 54
Sept.
8 17 43
20 14 31
Oct.
2 11 20
14 8 9
26 4 57
Nov.
7 1 46
18 22 35
30 19 24
Dec.
12 16 12
24 13 1

W Ursæ Maj.
$9^{h}\,37^{m}{\cdot}2$
$+56^{\circ}\,22'{\cdot}7$
7·9 to 8·7
Sixtieth.
$0^{d}\,4^{h}\,0^{m}{\cdot}2$.
Jan.
d h m
4 16 37
14 16 50
24 17 3
Feb.
3 17 16
13 17 30
23 17 43
March.
5 17 56
15 18 9
25 18 22
April.
4 18 36
14 18 49
24 19 2
May.
4 19 15
14 19 29
24 19 42
June.
3 19 55
13 20 8
23 20 22
July.
3 20 35
13 20 48
23 21 1
August.
2 21 14
12 21 28
22 21 41
Sept.
1 21 54
11 22 7
21 22 21
Oct.
1 22 34
11 22 47
21 23 0
31 23 13
Nov.
10 23 27
20 23 40
30 23 53
Dec.
11 0 6
21 0 20
31 0 33

R^2 Velorum.
$10^{h}\,18^{m}{\cdot}1$
$-41^{\circ}\,53'{\cdot}2$
10·0 to 10·9
Fourth.
$1^{d}\,20^{h}\,30^{m}{\cdot}0$.
Jan.
d h m
8 3 6
15 13 6
22 23 7
30 9 7
Feb.
6 19 7
14 5 7
21 15 7
March.
1 1 7
8 11 8
15 21 8
23 7 8
30 17 8
April.
7 3 8
14 13 8
21 23 9
29 9 9
May.
6 19 9
14 5 9
21 15 9
29 1 9
June.
5 11 9
12 21 10
20 7 10
27 17 10
July.
5 3 10
12 13 10
19 23 10
27 9 11
August.
3 19 11
11 5 11
18 15 11
26 1 11
Sept.
2 11 11
9 21 12
17 7 12
24 17 12
Oct.
2 3 12
9 13 12
16 23 12
24 9 13
31 19 13
Nov.
8 5 13
15 15 13
23 1 13
30 11 13
Dec.
7 21 14
15 7 14
22 17 14
30 3 14

Minima of Variable Stars of the Algol Type (continued).

R^3 Carinæ.	Z Draconis.	δ Libræ.	U Coronæ.	R Aræ.	U Ophiuchi.	V^2 Ophiu
$10^{h}\,54^{m}{\cdot}5$	$11^{h}\,40^{m}{\cdot}2$	$14^{h}\,56^{m}{\cdot}0$	$15^{h}\,14^{m}{\cdot}4$	$16^{h}\,32^{m}{\cdot}0$	$17^{h}\,11^{m}{\cdot}8$	$17^{h}\,30^{m}{\cdot}$
$-61^{\circ}\,25'{\cdot}1$	$+72^{\circ}\,46'{\cdot}0$	$-8^{\circ}\,9'{\cdot}0$	$+31^{\circ}\,59'{\cdot}2$	$-56^{\circ}\,48'{\cdot}5$	$+1^{\circ}\,18'{\cdot}8$	$+7^{\circ}\,18'{\cdot}$
12·2 to 12·8	9·5 to 13·6	5·0 to 6·2	7·5 to ...	6·9 to 8·0	6·0 to 6·7	8·5 to 10
Third.	*Sixth.*	*Fourth.*	*Third.*	*Second.*	*Tenth.*	*Second.*
$3^{d}\,7^{h}\,13^{m}$	$1^{d}\,8^{h}\,34^{m}{\cdot}7.$	$2^{d}\,7^{h}\,51^{m}{\cdot}4.$	$3^{d}\,10^{h}\,51^{m}{\cdot}2.$	$4^{d}\,10^{h}\,12^{m}{\cdot}1.$	$0^{d}\,20^{h}\,7^{m}{\cdot}7.$	$3^{d}\,16^{h}\,29$
Jan.	Jan.	Jan.	Jan.	Jan.	Jan.	Jan.
d h m	d h m	d h m	d h m	d h m	d h m	d h
0 15 17 10 12 56 20 10 35 30 8 14	0 21 26 9 0 54 17 4 22 25 7 50	2 13 45 11 21 10 21 4 36 30 12 1	0 11 39 10 20 12 21 4 46 31 13 20	2 14 15 11 10 40 20 7 4 29 3 28	1 3 46 9 13 2 17 22 19 26 7 35	2 3 5 9 12 5 16 21 5 24 6 4 31 15 4
Feb.	Feb.	Feb.	Feb.	Feb.	Feb.	Feb.
9 5 53 19 3 32	2 11 18 10 14 46 18 18 14 26 21 42	8 19 27 18 2 52 27 10 18	10 21 54 21 6 28	6 23 52 15 20 17 24 16 41	3 16 53 12 2 10 20 11 27 28 20 44	8 0 4 15 9 4 22 18 4
March.	March.	March.	March.	March.	March.	March.
1 1 11 10 22 50 20 20 29 30 18 8	7 1 10 15 4 39 23 8 7 31 11 35	8 17 43 18 1 9 27 8 35	3 15 2 13 23 35 24 8 9	5 13 5 14 9 29 23 5 54	9 6 1 17 15 18 26 0 35	2 3 4 9 12 4 16 21 4 24 6 4 31 15 4
April.	April.	April.	April.	April.	April.	April.
9 15 47 19 13 26 29 11 5	8 15 3 16 18 31 24 21 59	5 16 0 14 23 26 24 6 51	3 16 43 14 1 17 24 9 50	1 2 18 9 22 42 18 19 6 27 15 31	3 9 52 11 19 9 20 4 25 28 13 42	8 0 4 15 9 3 22 18 3 30 3 3
May.	May.	May.	May.	May.	May.	May.
9 8 44 19 6 23 29 4 2	3 1 27 11 4 55 19 8 23 27 11 51	3 14 17 12 21 42 22 5 8 31 12 33	4 18 24 15 2 58 25 11 32	6 11 55 15 8 19 24 4 44	6 22 59 15 8 16 23 17 33	7 12 3 14 21 3 22 6 3 29 15 3
June.	June.	June.	June.	June.	June.	June.
8 1 41 17 23 20 27 20 59	4 15 19 12 18 47 20 22 15 29 1 43	9 19 59 19 3 24 28 10 50	5 20 5 15 4 39 25 13 13	2 1 8 10 21 32 19 17 56 28 14 21	1 2 50 9 12 7 17 21 24 26 6 41	6 0 3 13 9 3 20 18 3 28 3 3
July.	July.	July.	July.	July.	July.	July.
7 18 38 17 16 17 27 13 56	7 5 11 15 8 39 23 12 7 31 15 36	7 18 15 17 1 41 26 9 6	5 21 47 16 6 20 26 14 55	7 10 45 16 7 9 25 3 33	4 15 58 13 1 15 21 10 31 29 19 48	5 12 3 12 21 2 20 6 2 27 15 2
August.	August.	August.	August.	August.	August.	August.
6 11 35 16 9 14 26 6 53	8 19 4 16 22 32 25 2 0	4 16 32 13 23 57 23 7 23	5 23 28 16 8 2 26 16 35	2 23 58 11 20 22 20 16 46 29 13 10	7 5 5 15 14 22 23 23 39	4 0 2 11 9 2 18 18 2 26 3 2
Sept.	Sept.	Sept.	Sept.	Sept.	Sept.	Sept.
5 4 32 15 2 12 24 23 51	2 5 28 10 8 56 18 12 24 26 15 52	1 14 48 10 22 14 20 5 39 29 13 5	6 1 9 16 9 43 26 18 17	7 9 35 16 5 59 25 2 23	1 8 56 9 18 13 18 3 30 26 12 47	2 12 2 9 21 2 17 6 2 24 15 2
Oct.	Oct.		Oct.	Oct.	Oct.	Oct.
4 21 30 14 19 9 24 16 48	4 19 20 12 22 48 21 2 16 29 5 44		7 2 50 17 11 24 27 19 58	3 22 47 12 19 12 21 15 36 30 12 0	4 22 4 13 7 21 21 16 38 30 1 55	2 0 1 9 9 1 16 18 1 24 3 1 31 12 1
Nov.	Nov.		Nov.	Nov.	Nov.	Nov.
3 14 27 13 12 6 23 9 45	6 9 12 14 12 40 22 16 8 30 19 36		7 4 32 17 13 6 27 21 39	8 8 25 17 4 49 26 1 13	7 11 12 15 20 29 24 5 49	7 21 1 15 6 1 22 15 1 30 0
Dec.	Dec.	Dec.	Dec.	Dec.		Dec
3 7 24 13 5 3 23 2 42	8 23 4 17 2 32 25 6 1	8 8 46 17 16 12 26 23 37	8 6 13 18 14 47 28 23 21	4 21 37 13 18 2 22 14 26 31 10 50		7 14 22

Minima of Variable Stars of the Algol Type (continued).

Z Herculis.	**S² Sagittarii.**	**V Serpentis.**	**X² Herculis.**	**W³ Sagitt.**	**R² Draconis.**	**U Scuti *.**
$17^{h}\,53^{m}{\cdot}9$	$18^{h}\,11^{m}{\cdot}4$	$18^{h}\,11^{m}{\cdot}5$	$18^{h}\,26^{m}{\cdot}4$	$18^{h}\,40^{m}{\cdot}1$	$18^{h}\,41^{m}{\cdot}0$	$18^{h}\,49^{m}{\cdot}3$
$+15^{\circ}\,8'{\cdot}8$	$-34^{\circ}\,8'{\cdot}4$	$-15^{\circ}\,33'{\cdot}3$	$+12^{\circ}\,32'{\cdot}8$	$-30^{\circ}\,35'{\cdot}4$	$+62^{\circ}\,34'{\cdot}9$	$-12^{\circ}\,43'{\cdot}3$
7·1 to 8·0	6·6 to 7·3	9·5 to 10·5	7·0 to 7·6	8·7 to 10·0	8·5 to 12·0	9·1 to 9·7
Fourth.	*Fourth.*	*Fourth.*	*Twelfth.*	*Fifth.*	*Fourth.*	*Tenth.*
{ $2^{d}\,1^{h}\,25^{m}$ 1 22 24	$2^{d}\,9^{h}\,58^{m}{\cdot}6$.	$3^{d}\,10^{h}\,53^{m}{\cdot}0$.	$0^{d}\,21^{h}\,20^{m}{\cdot}6$.	$2^{d}\,1^{h}\,50^{m}{\cdot}8$.	$2^{d}\,19^{h}\,56^{m}{\cdot}8$.	$0^{d}\,22^{h}\,55^{m}{\cdot}2$
Jan.	Jan.	Feb.	Jan.	Feb.	Jan.	Feb.
d h m	d h m	d h m	d h m	d h m	d h m	d h m
0 3 48	19 19 24	7 3 32	8 18 36	0 17 38	11 5 49	7 11 4
8 3 27	29 11 18	20 23 4	19 10 42	11 2 52	22 13 36	17 0 16
16 3 6	Feb.	March.	30 2 49	21 12 6	Feb.	26 13 27
24 2 45	8 3 13	6 18 36	Feb.	March.	2 21 23	March.
Feb.	17 19 7	20 14 8	9 18 56	3 21 20	14 5 10	8 2 39
1 2 24	27 11 2	April.	20 11 3	14 6 34	25 12 57	17 15 51
9 2 3	March.	3 9 40	March.	24 15 47	March.	27 5 3
17 1 42	9 2 56	17 5 12	3 3 10	April.	8 20 45	April.
25 1 21	18 18 50	May.	13 19 17	4 1 1	20 4 32	5 18 14
March.	28 10 45	1 0 44	24 11 24	14 10 15	31 12 19	15 7 26
5 1 1	April.	14 20 16	April.	24 19 29	April.	24 20 38
13 0 40	7 2 39	28 15 48	4 3 31	May.	11 20 6	May.
21 0 19	16 18 34	June.	14 19 38	5 4 43	23 3 53	4 9 49
28 23 58	26 10 28	11 11 20	25 11 45	15 13 57	May.	13 23 1
April.	May.	25 16 52	May.	25 23 11	4 11 41	23 12 13
5 23 37	6 2 23	July.	6 3 51	June.	15 19 28	June.
13 23 16	15 18 17	9 2 24	16 19 58	5 8 25	27 3 15	2 1 25
21 22 55	25 10 12	22 21 56	27 12 5	15 17 39	June.	11 14 36
29 22 34	June.	August.	June.	26 2 53	7 11 2	21 3 48
May.	4 2 6	5 17 28	7 4 12	July.	18 18 49	30 17 0
7 22 13	13 18 1	19 13 1	17 20 19	6 12 7	30 2 37	July.
15 21 53	23 9 55	Sept.	28 12 26	16 21 21	July.	10 6 11
23 21 32	July.	2 8 33	July.	27 6 35	11 10 24	19 19 23
31 21 11	3 1 49	16 4 5	9 4 33	August.	22 18 11	29 8 35
June.	12 17 44	29 23 37	19 20 40	6 15 49	August.	August.
8 20 50	22 9 38	Oct.	30 12 47	17 1 3	3 1 58	7 21 47
16 20 29	August.	13 19 9	August.	27 10 17	14 9 45	17 10 58
24 20 8	1 1 33	27 14 41	10 4 54	Sept.	25 17 33	27 0 10
July.	10 17 27	Nov.	20 21 0	6 19 31	Sept.	Sept.
2 19 47	20 9 22	10 10 13	31 13 7	17 4 45	6 1 20	5 13 22
10 19 26	30 1 16	24 5 45	Sept.	27 13 59	17 9 7	15 2 33
18 19 5	Sept.		11 5 14	Oct.	28 16 54	24 15 45
26 18 45	8 17 10		21 21 21	7 23 13	Oct.	Oct.
August.	18 9 5		Oct.	18 8 27	10 0 41	4 4 57
3 18 24	28 0 59		2 13 28	28 17 41	21 8 28	13 18 9
11 18 3	Oct.		13 5 35	Nov.	Nov.	23 7 20
19 17 42	7 16 54		23 21 42	8 2 55	1 16 16	Nov.
27 17 21	17 8 48		Nov.	18 12 9	13 0 3	1 20 32
Sept.	27 0 42		3 13 49	28 21 23	24 7 50	11 9 44
4 17 0	Nov.		14 5 56		Dec.	20 22 55
12 16 39	5 16 37		24 22 3		5 15 37	
20 16 18	15 8 32		Dec.		16 23 24	
28 15 57	25 0 26		5 14 9		28 7 12	
Oct.	Dec.		16 6 16			
6 15 36	4 16 21		26 23 23			
14 15 16						
22 14 55						
30 14 34						
Nov.						
7 14 13						
15 13 52						
23 13 31						
Dec.						
1 13 10						
9 12 49						
17 12 28						
[illegible] 8						

* It is doubtful whethe this star of the Al type.

Minima of Variable Stars of the Algol Type (continued).

V^1 LYRÆ.	U SAGITTÆ.	X^3 CYGNI.	Z^5 CYGNI.	V^3 CYGNI.	V^6 CYGNI.	Z^4 CYGNI
$19^{h}\ 12^{m}{\cdot}8$	$19^{h}\ 14^{m}{\cdot}7$	$19^{h}\ 43^{m}{\cdot}0$	$20^{h}\ 0^{m}{\cdot}8$	$20^{h}\ 4^{m}{\cdot}1$	$20^{h}\ 11^{m}{\cdot}6$	$20^{h}\ 19^{m}$
$+32^{\circ}\ 15'{\cdot}5$	$+19^{\circ}\ 26'{\cdot}4$	$+32^{\circ}\ 28'{\cdot}6$	$+41^{\circ}\ 19'{\cdot}4$	$+46^{\circ}\ 1'{\cdot}8$	$+34^{\circ}\ 13'{\cdot}1$	$+42^{\circ}\ 56'$
11·0 to 12·8	6·5 to 9·0	10·5 to 12·6	9·3 to 12·5	8·8 to 11·8	9·8 to 11·8	10·5 to 13
Fourth.	*Fourth.*	*Second.*	*Fourth.*	*Second.*	*Second.*	*Fourth.*
$3^{d}\ 14^{h}\ 22^{m}{\cdot}6$.	$3^{d}\ 9^{h}\ 8^{m}{\cdot}1$.	$6^{d}\ 0^{h}\ 8^{m}{\cdot}6$.	$3^{d}\ 7^{h}\ 37^{m}{\cdot}6$.	$4^{d}\ 13^{h}\ 45^{m}{\cdot}0$.	$8^{d}\ 10^{h}\ 20^{m}{\cdot}3$.	$3^{d}\ 10^{h}\ 49^{m}$
Jan.	Jan.	Jan.	Jan.	Jan.	Jan.	Jan.
d h m	d h m	d h m	d h m	d h m	d h m	d h m
0 11 11 14 20 41 29 6 11	0 15 43 14 4 15 27 16 47	5 8 24 17 8 42 29 8 59	2 12 10 15 18 40 29 1 11	0 12 32 9 16 2 18 19 32 27 23 2	3 11 46 20 8 27	2 5 21 16 0 37 29 19 54
Feb.	Feb.	Feb.	Feb.	Feb.	Feb.	Feb.
12 15 41 27 1 12	10 5 20 23 17 52	10 9 16 22 9 33	11 7 41 24 14 12	6 2 32 15 6 2 24 9 32	6 5 7 23 1 48	12 15 11 26 10 28
March.	March.	March.	March.	March.	March.	March.
13 10 42 27 20 13	9 6 25 22 18 57	6 9 50 18 10 8 30 10 25	9 20 42 23 3 12	5 13 2 14 16 33 23 20 3	11 22 28 28 19 9	12 5 44 26 1 1
April.	April.	April.	April.	April.	April.	April.
11 5 43 25 15 13	5 7 29 18 20 2	11 10 42 23 10 59	5 9 43 18 16 13	1 23 33 11 3 3 20 6 33 29 10 3	14 15 49	8 20 18 22 15 35
May.	May.	May.	May.	May.	May.	May.
10 0 43 24 10 14	2 8 34 15 21 7 29 9 39	5 11 16 17 11 34 29 11 51	1 22 44 15 5 14 28 11 44	8 13 33 17 17 3 26 20 33	1 12 30 18 9 11	6 10 52 20 6 8
June.	June.	June.	June.	June.	June.	June.
7 19 44 22 5 14	11 22 11 25 10 44	10 12 8 22 12 25	10 18 15 24 0 45	5 0 3 14 3 33 23 7 3	4 5 51 21 2 32	3 1 25 16 20 42 30 15 59
July.	July.	July.	July.	July.	July.	July.
6 14 45 21 0 15	8 23 16 22 11 49	4 12 42 16 13 0 28 13 17	7 7 16 20 13 46	2 10 33 11 14 3 20 17 34 29 21 4	7 23 12 24 19 53	14 11 15 28 6 32
August.	August.	August.	August.	August.	August.	August.
4 9 45 18 19 16	5 0 21 18 12 53	9 13 34 21 13 51	2 20 16 16 2 47 29 9 17	8 0 34 17 4 4 26 7 34	10 16 33 27 13 14	11 1 49 24 21 6
Sept.	Sept.	Sept.	Sept.	Sept.	Sept.	Sept.
2 4 46 16 14 16 30 23 47	1 1 26 14 13 58 28 2 31	2 14 8 14 14 26 26 14 43	11 15 48 24 22 18	4 11 4 13 14 34 22 18 4	13 9 55 30 6 35	7 16 23 21 11 39
Oct.	Oct.	Oct.	Oct.	Oct.	Oct.	Oct.
15 9 17 29 18 47	11 15 3 25 3 35	8 15 0 20 15 17	8 4 48 21 11 19	1 21 34 11 1 4 20 4 34 29 8 4	17 3 16	5 6 56 19 2 13
Nov.	Nov.	Nov.	Nov.	Nov.	Nov.	Nov.
13 4 18 27 13 48	7 16 8 21 4 40	1 15 34 13 15 52 25 16 9	3 17 49 17 0 20 30 6 50	1 11 34 16 15 4 25 18 34	2 23 56 19 20 37	1 21 30 15 16 46 29 12 3
Dec.	Dec.	Dec.	Dec.	Dec.	Dec.	Dec.
11 23 18 26 8 48	4 17 13 18 5 45	7 16 26 19 16 43	13 13 20 26 19 51	4 22 4 14 1 35 23 5 5	6 17 17 23 13 58	13 7 20 27 2 37

Minima of Variable Stars of the Algol Type (continued).

W Delphini.	**R² Delphini.**	**Y Cygni.**	**U⁵ Cygni.**	**Y⁵ Cygni.**	**T⁶ Cygni.**	**T⁵ Cygni.**
$20^h\ 33^m{\cdot}4$ $+17^\circ\ 57'{\cdot}5$ 9·5 to 12·0	$20^h\ 39^m{\cdot}2$ $+13^\circ\ 36'{\cdot}6$ 9·5 to 10·5	$20^h\ 48^m{\cdot}4$ $+34^\circ\ 18{\cdot}5$ 7·1 to 7·9	$21^h\ 2^m{\cdot}6$ $+45^\circ\ 24'{\cdot}3$ 11·0 to 12·5	$21^h\ 48^m{\cdot}0$ $+42^\circ\ 41'{\cdot}8$ 8·2 to 9·2	$21^h\ 18^m{\cdot}9$ $+41^\circ\ 59'{\cdot}9$ 8·5 to 10·0	$21^h\ 55^m{\cdot}5$ $+43^\circ\ 53'{\cdot}8$ 8·9 to 11·6
Second. $4^d\ 19^h\ 20^m{\cdot}9$.	*Second.* $4\ \ 14^h\ 25^m{\cdot}4$.	*Sixth.*	*Eighth* $1^d\ 11^h\ 27^m{\cdot}0$.		*Twentieth.* $0^d\ 14^h\ 1^m{\cdot}6$.	
Jan. d h m 1 17 38 11 8 19 20 23 1 30 13 43	Jan. d h m 0 8 43 9 13 34 18 18 25 27 23 16	Jan. d h m 0 11 15 9 10 58 18 10 41 27 10 24	Jan. d h m 0 7 48 12 3 24 23 22 59	Jan. d h m 9 8 36 19 2 3 28 19 30	Jan. d h m 5 20 0 17 12 32 29 5 4	Jan. d h m 14 14 50
Feb. 9 4 25 18 19 7 28 9 48	Feb. 6 4 7 15 8 57 24 13 48	Feb. 5 10 7 14 9 51 23 9 34	Feb. 4 18 35 16 14 11 28 9 46	Feb. 7 12 57 17 6 23 26 23 50	Feb. 9 21 37 21 14 10	Feb. 14 22 8
March. 10 0 30 19 15 12 29 5 54	March. 5 18 39 14 23 30 24 4 21	March. 4 9 17 13 9 0 22 8 43 31 8 26	March. 12 5 22 24 0 58	March. 8 17 17 18 10 44 28 4 11	March. 5 6 42 16 23 15 28 15 47	March. 18 5 26
April. 7 20 36 17 11 18 27 1 59	April. 2 9 12 11 14 3 20 18 54 29 23 45	April. 9 8 9 18 7 52 27 7 36	April. 4 20 33 16 16 9 28 11 44	April. 6 21 38 16 15 5 26 8 32	April. 9 8 20 21 0 53	April. 18 12 44
May. 6 16 41 16 7 23 25 22 5	May. 9 4 35 18 9 26 27 14 17	May. 6 7 19 15 7 2 24 6 45	May. 10 7 20 22 2 56	May. 6 1 58 15 19 25 25 12 52	May. 2 17 25 14 9 58 26 2 30	May. 19 20 1
June. 4 12 46 14 3 28 23 18 10	June. 5 19 8 14 23 59 24 4 50	June. 2 6 28 11 6 11 20 5 54 29 5 37	June. 2 22 31 14 18 7 26 13 43	June. 4 6 19 13 23 46 23 17 13	June. 12 15 19 24 7 52	June. 20 3 19
July. 3 8 52 12 23 34 22 14 15	July. 3 9 41 12 14 32 21 19 22 31 0 13	July. 8 5 21 17 5 4 26 4 47	July. 8 9 18 20 4 54	July. 3 10 40 13 4 7 22 21 33	July. 6 0 24 17 16 57 29 9 29	July. 21 10 37
August. 1 4 57 10 19 39 20 10 21 30 1 3	August. 9 5 4 18 9 55 27 14 46	August. 4 4 30 13 4 13 22 3 56 31 3 39	August. 1 0 29 12 20 5 24 15 41	August. 1 15 0 11 8 27 21 1 54 30 19 21	August. 10 2 2 21 18 35	Aug. 21 17 55
Sept. 8 15 44 18 6 26 27 21 8	Sept. 5 19 37 15 0 28 24 5 19	Sept. 9 3 22 18 3 6 27 2 49	Sept. 5 11 16 17 6 52 29 2 28	Sept. 9 12 48 19 6 15 28 23 42	Sept. 2 11 7 14 3 40 25 20 12	Sept. 22 1 12
Oct. 7 11 50 17 2 32 26 17 13	Oct. 3 10 9 12 15 0 21 19 51 31 0 42	Oct. 6 2 32 15 2 15 24 1 58	Oct. 10 22 3 22 17 39	Oct. 8 17 9 18 10 35 28 4 2	Oct. 7 12 45 19 5 17 30 21 50	Oct. 23 8 30
Nov. 5 7 55 14 22 37 24 13 19	Nov. 9 5 33 18 10 24 27 15 15	Nov. 2 1 41 11 1 24 20 1 7 29 0 50	Nov. 3 13 14 15 8 50 27 4 26	Nov. 6 21 29 16 14 56 26 8 23	Nov. 11 14 22 23 6 55	Nov. 23 15 48
Dec. 4 4 0 13 18 42 23 9 24	Dec. 6 20 6 16 0 57 25 5 47	Dec. 8 0 34 17 0 17 26 0 0	Dec. 9 0 1 20 19 37	Dec. 6 1 50 15 19 17 25 12 44	Dec. 4 23 28 16 16 0 28 8 33	Dec. 24 23 6
					β Lyræ type.	

Y Cygni: alternate periods are $1^d\ 12^h\ 38^m$ and $1^d\ 11^h\ 16^m$ in January,

MAXIMA OF Y^3 CYGNI.

$20^{h}\ 29^{m}{\cdot}9 \qquad +46^{\circ}\ 17'{\cdot}0 \qquad 8{\cdot}7$ to $10{\cdot}0$

Jan.	Mar.	May.	July.	Sept.	Nov.
d h	d h	d h	d h	d h	d h
10 10	11 18	11 2	10 11	8 19	8 3
25 12	26 20	26 4	25 13	23 21	23 5
Feb.	April.	June.	August.	Oct.	Dec.
9 14	10 22	10 6	9 15	8 23	8 7
24 16	26 0	25 9	24 17	24 1	23 9

This star remains constant for 3 or 4 days at minimum.

MAXIMA OF S^5 CYGNI.

$20^{h}\ 52^{m}{\cdot}6 \qquad +30^{\circ}\ 4'{\cdot}4 \qquad 9{\cdot}7$ to $10{\cdot}4$

Jan.	Mar.	May.	July.	Sept.	Nov.
d h m	d h m	d h m	d h m	d h m	d h m
3 3 24	4 16 46	4 6 7	3 19 29	2 8 51	1 22 13
9 20 53	11 10 15	10 23 37	10 12 58	9 2 20	8 15 42
16 14 23	18 3 44	17 17 6	17 6 28	15 19 49	15 9 11
23 7 52	24 21 13	24 10 35	23 23 57	22 13 18	22 2 40
30 1 21	31 14 42	31 4 4	30 17 26	29 6 47	28 20 9
Feb.	Apr.	June.	August.	Oct.	Dec.
5 18 50	7 8 11	6 21 33	6 10 55	6 0 16	5 13 38
12 12 19	14 1 40	13 15 2	13 4 24	12 17 45	12 7 7
19 5 48	20 19 10	20 8 31	19 21 53	19 11 15	19 0 36
25 23 17	27 12 39	27 2 0	26 15 22	26 4 44	25 18 5

Period $13^{h}\ 27^{m}{\cdot}4$. Every twelfth maximum is given. The minima happen $1^{h}\ 23^{m}$ before the maxima.

MAXIMA OF V^2 CAPRICORNI.

$20^{h}\ 56^{m}{\cdot}3 \qquad -15^{\circ}\ 35'{\cdot}5 \qquad 9{\cdot}2$ to $10{\cdot}1$

Mar.	May.	July.	Sept.	Nov.
d h m	d h m	d h m	d h m	d h m
12 16 33	5 0 39	7 1 35	7 17 31	9 9 27
21 15 24	14 8 30	16 0 26	16 16 22	18 8 18
30 14 15	23 7 20	24 23 17	25 15 13	27 7 9
Apr.	June.	August.	Oct.	Dec.
8 13 6	1 6 11	2 22 7	4 14 4	6 6 0
17 11 57	10 5 2	11 20 58	13 12 54	15 4 51
26 10 48	19 3 53	20 19 49	22 11 45	24 3 44
	28 2 44	29 18 40	31 10 36	

Period $10^{h}\ 44^{m}{\cdot}5$. Every twentieth maximum given. The star remains constant at minimum for 3 hours.

MAXIMA OF V LACERTÆ.

$22^{h}\ 44^{m}{\cdot}8 \qquad +55^{\circ}\ 49'{\cdot}8 \qquad 8{\cdot}5$ to $9{\cdot}5$

Jan.	Mar.	May.	July.	Sept.	Nov.
d h	d h	d h	d h	d h	d h
2 15	3 11	2 6	1 1	8 20	7 15
12 15	13 10	12 5	11 0	18 19	17 14
22 14	23 9	22 4	21 0	28 18	27 13
			30 23		
Feb.	April.	June.	August.	Oct.	Dec.
1 13	2 8	1 4	9 22	8 17	7 13
11 12	12 8	11 3	19 21	18 16	17 12
21 11	22 7	21 2	29 20	28 16	27 11

Period $4^{d}\ 23^{h}{\cdot}6$. Alternate maxima are given. The minina happen 1 day before the maxima.

DATES OF MAXIMA OF VARIABLE STARS.

nuary.

T Lib.
W Peg.
U Lib.
U^3 Scor.
Z Cam.
R Ari.
U C. Min.
S Apo.
V Lyr.
U Drac.
Z Aq.
U Mon.
S Touc.
T^2 Peg.
W Ori.
V^2 Cyg.
W^2 Oph.
Y Gem.
J Ari.
^{2} Peg.
X Cap.
R^2 Pers.
T Herc.
W Hyd.
V Scu.
T$_2$ Sgt.
U$_3$ Lyr.
U Car.
W Cet.
R Cap.
S^2 Scor.
R U. Maj.
X^2 And.
T Cent.
U U. Min.
Y^2 Scor.
T^3 Her.
Z^2 Cyg.
V Peg.
Z Lyr.
S Drac.
Z Aqu.
V^6 Cyg.
T Hor.
T Eri.
U Pup.
S^2 Cyg.
S^2 Lib.
R Scul.
U Cap.
T C. Min.
R Sgt.
W^2 Aur.
U Cam.
R Ori.
l Car.
S Lib.
S Lac.
R Lup.
^{8} *Cyg.*

January (con.).

d	
28	Z Ceph.
	S P. Aus.
	W^2 Scor.
29	R^2 Aqu.
	X Del.
30	R^2 Car.
	V^2 Aur.
	S Del.
	W Lib.
31	S Scor.

February.

d	
1	V Sag.
	Z Cam.
	R Cha.
	U Eri.
2	Y Mon.
4	V Boö.
5	S^2 Pup.
	R Lyr.
7	X Ori.
	V^2 Sgt.
	R Cæ.
	R Aq.
8	W Pup.
	Y^2 Scor.
9	U Ser.
10	Z Aur.
	W Ori.
	R^3 Aq.
	V Cor.
	S Leo.
11	U Gem.
	S Tau.
	Y Cap.
	X Her.
12	Y Scor.
13	U Vel.
	R^3 And.
17	X Eri.
	R Car.
18	S Cor.
19	Y^2 Lyr.
20	Z Pup.
21	V Cap.
22	V Aqu.
	V Can.
	S Vul.
	V^3 Aq.
23	U Car.
	U Mon.
	W Cen.
	Y Cass.
24	χ Cyg.
	L^2 Pup.
	T C. Maj.
25	Y Virg.
26	*T Gru.*

February (con.).

d	
26	R Pav.
	S Cyg.
28	Y^2 Scor.

March.

d	
1	Z Hyd.
	R Lep.
2	R C. Aus.
	Z Cam.
	S Sgt.
3	S Lyr.
	R^2 Tau.
	l Car.
	X^2 And.
4	R Oph.
5	Y Vel.
	Z Car.
	R Sag.
6	S^2 Gem.
	T Cam.
7	R Pi. A.
9	T Oct.
10	W^3 Lyr.
	T Hyd.
	Z^2 Sgt.
	Z Sgt.
12	R Leo.
	V Tau.
14	W Ori.
	R C. Min.
15	T For.
	W Her.
	U^2 Sgt.
	V Cas.
16	Y And.
	U Virg.
17	V Ori.
	W Cyg.
18	Z Del.
	S^2 Pup.
	X Lib.
19	S^2 Cent.
20	U^2 Scor.
21	W^2 And.
	Y^2 Scor.
	T Touc.
22	U^2 Cap.
	T Serp.
23	T Apo.
	R Lyr.
24	Z^2 Scor.
25	Y Ori.
	T Scul.
	R^2 Cap.
	Y^2 Oph.
	S Herc.
	U Oct.
26	R Pers.

March (con.).

d	
27	T^3 Scor.
	T Sag.
	V Delp.
	S Cet.
28	S Aq.
29	S^2 Aqu.
	W Leo.
30	X Gem.
	W^2 Car.
	V^2 Aur.

April.

d	
1	U Cen.
	Z Cam.
	R^2 Peg.
2	T^2 Scor.
	U Car.
3	T U. Maj.
4	W Eri.
5	W Cor.
	S Oct.
6	R^2 Aq.
7	W Cap.
	l Car.
9	S Pav.
	R Mic.
10	U Mon.
	Y^2 Scor.
11	V^2 And.
12	Z Tau.
	R Vul.
13	R^3 Her.
	U^2 And.
15	W Ori.
16	U Vel.
	R^2 Virg.
	U$_3$ Aq.
17	X$_2$ And.
18	W Aur.
	X Cam.
	U Pav.
	T Cen.
19	U Mic.
20	U C. Aus.
	T^4 Cyg.
21	T^2 Drac.
23	S Phœ.
	T Gem.
	U Scul.
24	R Cyg.
	V Gem.
25	U^2 Ceph.
	V^3 Scor.
	Y Cen.
	Y Ori.
	Z Virg.
26	Y^2 Aq.
	T^2 Lyr.

April (con.).

d	
27	V^3 Lyr.
	R Gru.
28	T Cet.
29	S^2 Pap.
	V Aur.
30	Z Cam.
	Y^2 Scor.
	S Vul.

May.

d	
1	W Scor.
2	R Virg.
3	X Cet.
	Z^4 Aq.
4	T Nor.
5	V^2 Aq.
	X Cas.
7	X Peg.
	R Equ.
8	U Gem.
	V Sag.
	S^2 Hyd.
	W Mon.
9	S Car.
	R Lyr.
	U Cas.
10	Z Hyd.
	R Herc.
11	X Scor.
	Z Peg.
	U Car.
13	*l* Car.
14	T And.
	Z Aq.
15	R Sag.
16	R Col.
	X Herc.
17	U Pers.
18	W Ori.
	U^2 Virg.
	R Boö.
19	U Aqu.
	R^6 Cyg.
20	U And.
	V And.
21	Y^2 Scor.
	U^2 Pers.
22	R Touc.
	U Boö.
23	T Pav.
	X^2 Lyr.
24	X Aur.
25	R Aqu.
26	R^2 Mon.
	S^2 Drac.
	T C. Ven.
	T Cap.
	U Mon.

May (con.).

d	
27	R Oct.
28	R Pict.
	S Ari.
	V^2 Aur.
	S Ori.
	R Cet.
29	S Gru.
30	Z Cam.
	R C. Aus.
	U^3 Herc.
31	U^2 Oph.
	R^2 Lib.

June.

d	
1	R^4 Cyg.
	R^2 Herc.
	X^2 And.
2	T Del.
	Z Lib.
	R Scor.
3	Z Aur.
4	S Oph.
5	R Pis.
	T U. Min.
6	R^2 Tau.
	W Cam.
	U^2 Herc.
	T^2 Cam.
8	R C. Ven.
9	W Pup.
	S^2 Pup.
	S Aqu.
10	Y^2 Scor.
11	R Lib.
	V^2 Car.
	T Phœ.
13	R Mali.
14	R^3 And.
	V Virg.
15	T For.
	Z^2 Car.
16	T^2 Aq.
	T Cep.
17	U Vel.
	l Car.
18	U^2 Lib.
	X Cen.
19	U Car.
	W Ori.
21	Y^2 Herc.
	R^2 Sgt.
	W Lyr.
22	S^3 Lyr.
	U Pisc.
23	S^6 Cyg.
	Z Boö.
	X^2 Sgt.
	W^3 Cyg.

June (con.).

d	
24	R Lyr.
25	U Ori.
	R^2 Op
26	S Col.
	T^2 Cyg
	T Herc
	S U. M
28	U Her
	W Lu
	Z Cam.
29	R Com
	R And
30	Y^2 Sco

July.

d	
3	X Ori.
	X Cep.
	V Oph
4	V^2 He
5	Z Cyg.
7	Y Peg.
	S Vul.
8	W And
9	X Oph.
	R Ari.
	V Lib.
11	R Drac
	S Peg.
	U Mon
12	T Ari.
	V Ceti.
13	T Gru.
	Y Pers.
	S Mic.
14	T Aqu.
	L^3 Pup
	T^3 Aq.
15	X^6 Cyg.
	S Boö.
16	X^2 And
17	T^2 Cen.
	V Drac.
	T Cen.
18	U Ceti.
	X Aqu.
19	Z Hyd.
20	S^2 Pup.
21	Y^2 Scor.
	R^2 Scor.
	W Ori.
	W Vel.
22	W^2 Lyr.
23	*l* Car.
24	R Sag.
25	T Sgt.
	R Gem.
26	V^2 Aur.
	R^3 Herc

Aq.=Aquilæ. *Aqu.=Aquarii.* C. Aus.=Coronæ Australis. Can.=Cancri.

Maxima of Variable Stars (continued).

July (con.).

d	
27	S Mali.
	T² Virg.
	W Cyg.
	S Scor.
	W² Aur.
	X² Aq.
28	U Car.
	Z Cam.
	R Ind.
	V Cap.
	Z Ceti.
29	S² Gem.
30	U Canc.
31	S² Herc.

August.

d	
1	R Cam.
	T² Herc.
2	U Gem.
	W³ Aq.
3	W Aqu.
	R Phœ.
	R Serp.
5	V² Peg.
6	U² Aq.
	S C. Min.
	S Lib.
7	R Tri.
	R² Aqu.
8	X Hyd.
9	T Col.
	Z Cap.
10	R Lyr.
	S U. Min.
	X And.
	Y² Scor.
11	T² Peg.
12	V Sag.
13	R Hyd.
	R² Cas.
	R² And.
16	U Cap.
	U Lib.
	S Ind.
17	X Scul.
	X Herc.

August (con.).

d	
18	W Sag.
	U Vel.
	Y Drac.
19	U³ Lyr.
	S Leo.
20	T² Gem.
	V Cam.
	R Cor.
21	X Cap.
	S Hor.
22	S Aq.
	Z Oph.
23	W Ori.
	S Hyd.
24	W Lib.
25	Z Aqu.
	Y Lib.
	X U. Maj.
	R Mic.
26	R Vul.
	U Mon.
	U² Oph.
	Z Cam.
	U² Aur.
27	T Lib.
	R C. Aus.
	l Car.
28	T Hor.
	S Lupi.
29	R³ Aq.
	V Mon.
	X Lib.
	V Tau.
30	S² Cen.
	V C. Min.
	S Gem.
	Y² Scor.
	T² Drac.
31	S² Pup.
	V⁴ Cyg.
	X² And.
	V Cruc.

September.

d	
1	S² Lib.
3	X² Pers.

September (con.).

d	
3	W Canc.
4	T² Lib.
	Z² Scor.
	U Car.
	S Touc.
5	Y Cap.
7	R⁶ Cyg.
8	T Sag.
	R Cas.
9	R² Tau.
	U² Lyr.
10	X² Car.
	X Cam.
	S L. Min.
11	V Leo.
12	W Cas.
	S Vul.
13	W² Oph.
14	W Cen.
	U C. Aus.
	R Del.
15	T For.
16	T Oph.
	S² Aq.
	U³ Scor.
	Y Mon.
	U Aur.
18	R Tau.
	Z Aq.
19	R Lupi.
	R Ret.
20	Y² Scor.
	W Lac.
21	W. Oph.
	S Lac.
23	V² Aur.
24	Z Aur.
	S Phœ.
	W Ori.
25	Z Can.
	R Lyr.
	R Virg.
26	V Scu.
	R For.
	T Cas.
27	Z Hyd.
28	U Eri.

September (con.).

d	
28	V Scul.
29	U² Cep.
	S Lyn.
30	T Eri.

October.

d	
2	Y Virg.
	l Car.
	R Lac.
	R Sag.
4	S Pic.
	S Car.
6	T Peg.
7	T Ceti.
	U Ser.
	W Pup.
8	V² Cep.
	X² Cen.
	U Virg.
	Z² Sgt.
9	V Cyg.
10	V² And.
	Y² Scor.
	T Drac.
11	S Virg.
	S² Pup.
	U Mon.
12	S Cam.
	R Peg.
13	T Oct.
	R³ And.
	U Car.
14	W² Cyg.
	Y And.
	V³ Sgt.
	R Pav.
	U² Hyd.
15	X² And.
16	T Scul.
	T Cen.
17	T. Virg.
	R Sgt.
18	V Boö.
19	U Vel.
	Z² Cyg.

October (con.).

d	
20	R² Cep.
	R L. Min.
21	S Sgt.
22	S Serp.
23	Y Gem.
	Z Scor.
24	R Pers.
25	Z Cam.
26	V Aur.
	W Ori.
27	S Pis. A.
28	U Gem.
	R Canc.
	X Ceti.
29	S² Aur.
	S Apo.
30	V² Aq.
	Y² Scor.

November.

d	
1	W Cap.
2	X Del.
	X Aur.
	V Cas.
	R² Hyd.
3	V Hyd.
	R² Aur.
	S Del.
4	X Eri.
	T Lyn.
	U Cen.
6	Z Lyr.
	l Car.
7	R³ Herc.
8	Y Cep.
9	Z Cep.
	T² Drac.
10	R Lyr.
11	R⁵ Cyg.
	R Cet.
	T² Sgt.
12	U² Sgt.
13	R Pic.
14	S Drac.
15	R Dor.

November (con.).

d	
15	U Boö.
	o Cet.
16	V Sag.
	R U. Maj.
17	V⁶ Cyg.
	S³ Cyg.
1	V Peg.
	Y Aqu.
	U² Pers.
19	Z Vel.
	R² Virg.
	X Herc.
	S Val.
20	Y² Scor.
	R² Cap.
	S² Virg.
21	U Car.
	V Canc.
	V² Aur.
	S² Pup.
22	W Cor.
	X Peg.
23	T² Oph.
	Z Cam.
24	S Scul.
	R C. Aus.
26	X Scor.
	U Mon.
27	T Gru.
	T Touc.
28	W Ori.
	T Lep.
29	X² And.
30	U U. Min.
	X Aq.
	S Lyr.

December.

d	
1	L² Pup.
	Y Cen.
3	U Pup.
5	U Drac.
	W Cyg.
6	Z Hyd.
	Z² Herc.
7	T Herc.

Decem

(con.

d	
8	V³ L
	W P
9	V O
	T C.
	W S
10	Y² S
11	V C
12	*l* Ca
	R S
	V² A
	U P
13	R² T
14	W³
	S² S
15	R C
	U² A
	T³ H
16	T F
17	T A
18	Y O
	W
	U³
	U² T
19	S O
	T U
20	U V
	X G
21	S² G
23	Z C
	U²
	T H
24	S² L
	R P
	V² S
25	X²
	R C
	R T
26	W
	R B
	R L
	R L
29	U C
30	R²
	W
	Y² S
31	Y D
	R O

DATES OF MINIMA OF VARIABLE STARS.

January.

d	
5	S² Gem.
6	Z² Oph.
7	Z² Scor.
	V Vul.
8	V Oph.
	S² Cen.
	X⁶ Cyg.
	U Herc.

January (con.).

d	
8	TCan.Ven.
9	U Car.
10	W Cap.
11	*η* Gem.
12	Y² Scor.
13	T Oct.
	R Pis.
	W Cyg.

January (con.).

d	
13	*l* Car.
	R Lyr.
14	U Vel.
15	S Aq.
16	T Norm.
21	R C. Aus.
22	T Cas.
23	X Cet.

January (con.).

d	
23	S² Pup.
25	W Ori.
	V² And.
26	S Vul.
28	U Ori.
	X Cen.
29	U Cas.

February.

d	
1	R² Sag.
	T For.
	Y² Scor.
3	X Peg.
4	R Hyd.
	R Mic.
	W Aqu.
5	T Sag.

February (con.).

d	
5	U Mon.
6	R Boö.
7	S Phœ.
9	R Vul.
11	Z Hyd.
12	T Pav.
	V Dra.
14	V Vul.

Febr

(co

d	
15	X C
16	U B
	W
	R S
17	U C
	U C
18	*l* Ca
19	T²

C. Maj.=Canis Major. *Cœ.=Cœlum.* Cha.=Chamælon. For.=Fornacis. U. Min.=Urs

Minima of Variable Stars (continued).

February (con.).

d
0 R^2 And.
1 Y^2 Scor.
3 R Vir.
S Peg.
S C. Min.
5 S Car.
6 W^2 Cyg.
W Ori.
7 V^4 Cyg.

March.

1 R And.
R Comæ.
R Lyr.
2 Z Cyg.
3 V Cyg.
4 R^3 Herc.
5 R Ser.
S^2 Pup.
S Boö.
7 T Ari.
S U. Min.
Z Oph.
8 R^2 Cas.
Y Pers.
X Aur.
0 X Hyd.
R Cas.
Z Aq.
S U. Maj.
1 U^2 Pers.
2 V Lib.
R Cam.
V U. Maj.
3 R^2 Scor.
X And.
Y^2 Herc.
R^4 Cyg.
4 Y^2 Scor.
7 U Vel.
X Herc.
8 U Cet.
9 R Cet.
T^2 Cen.
2 V Mon.
T Cen.
W Lyr.
3 U Mon.
V Vul.
5 R Drac.
l Car.
6 R Phœ.
R Gem.
8 U Car.
V^2 Herc.
0 T^2 Cyg,
W Ori.
U Pis.

April.

d
3 Y^2 Scor.
4 S Vul.
6 R Tri.
7 Z Cap.
8 T Herc.
9 R Pic.
R Ari.
11 Z Aur.
T Drac.
13 R^2 Aqu.
14 T Lyn.
16 W Pup.
S^2 Pup.
R Lyr.
17 T Aqu.
19 S Lyr.
20 R C. Aust.
22 Z Hyd.
23 R Peg.
Y^2 Scor.
24 S Mic.
S^2 Lib.
25 X Cap.
26 T. Col.
28 R Sag.
30 *l* Car.
V Vul.

May.

1 T Gru.
R Tau.
2 S Gem.
W Ori.
3 S Cam.
4 T For.
5 Z Cet.
S Cet.
6 S Virg.
U Car.
7 Y Cep.
R Del.
T Hor.
8 U Mon.
R L. Min.
9 R For.
14 R Ind.
Y^2 Scor.
T Lib.
15 S Hyd.
16 L^2 Pup.
17 T Vir.
18 S. Leo.
U Vel.
21 V Cep.
23 S Cep.
24 S Drac.
W Cyg.
25 *Y Cap.*

May (con.).

d
25 R Lup.
26 Y Mon.
27 U Eri.
S^2 Pup.
29 V Tau.
30 S Lyn.
S^2 Gem.
S Lac.

June.

1 V^2 Cyg.
R Sgt.
2 R Lyr.
3 Y^2 Scor.
W Ori.
R Nor.
4 *l* Car.
V Vulp.
9 X Aq.
10 X Lib.
S Vul.
S Aq.
12 T Eri.
13 U Car.
16 R^3 Herc.
S Scul.
W Cen.
17 T Lep.
R Aur.
18 U Cyg.
X Herc.
20 T Cen.
2 Z^2 Scor.
S Touc.
S^2 Cen.
22 R Mic.
2 Z C. Aust.
U Mon.
Y^2 Scor.
24 R Lyn.
R Canc.
R Vul.

July.

1 Z Hyd.
4 Y And.
T^2 Sgt.
5 X Delp.
W Ori.
6 W Peg.
7 R Sag.
S^2 Pup.
8 V Boö.
S Delp.
9 Y Virg.
V Aqu.

July (con.).

d
9 U Drac.
10 *l* Car.
X Cam.
11 S Sgt.
S Phœ.
Z^2 Sgt.
12 U C. Aust.
U Virg.
o Cet.
14 Y^2 Scor.
15 V Vul.
Z Aq.
6 R Pav.
8 V Cas.
R Lyr.
R C. Aust.
19 U Vel.
R Virg.
20 T Sag.
R Per.
X Cet.
21 U Cen.
22 U Car.
S Car.
23 T Scul.
X Gem.
24 R^2 Pers.
W Hyd.
26 V Peg.
V^2 And.
28 V Canc.
29 R U. Maj.
30 U^2 Sgt.
W Cor.

August.

1 W Scor.
2 Z Aur.
3 Y^2 Scor.
4 T For.
5 S^2 Scor.
6 W Cap.
V Ori.
7 S Cyg.
W Ori.
R Dor.
8 U Mon.
12 U Boö.
W Herc.
14 *l* Car.
W Pup.
15 V Cor.
U^2 Gem.
17 R^2 Scul.
X Aur.
S Vul.
18 S^2 Pup.

August (con.).

d
19 Z Delp.
T Oct.
R Aq.
21 R Car.
R Cen.
X Peg.
22 V Vul.
23 Y^2 Scor.
R Ori.
24 S Pis.
26 R Horo.
28 V Lyr.
T And.
R Leo.
S Herc.
30 U^2 And.
U Car.
η Gem.

September.

2 R Cet.
R Lyr.
S Oct.
T U. Maj.
8 W Ori.
U^2 Per.
9 Z Hyd.
Y^2 Scor.
T Gru.
V Gem.
R Sag.
16 U C. Min.
R Boö.
17 T Cam.
W Mon.
T Nor.
18 T Cen.
19 U Vel.
l Car.
S Cet.
T Herc.
20 X Herc.
U Pis.
22 S Cas.
2 T Cap.
U Mon.
24 Z^2 Oph.
2 R Pic.
2 R^3 Herc.
S^2 Pup.
V Vul.
30 V U. Maj.

October.

2 S Ari.
R^2 Car.

October (con.).

d
2 U Oct.
3 Y^2 Scor.
W Cyg.
L^2 Pup.
5 W Lyr.
6 X Cas.
. T^2 Cyg.
7 U Car.
8 R C. Min.
10 R Lep.
W Ori.
12 R Ari.
14 T Pav.
R^4 Cyg.
V^2 Herc.
15 R Col.
S Cor.
R C. Aust.
16 T C. Ven.
17 *χ* Cyg.
U Per.
19 R Ly
20 R^2 Aqu.
21 Y^2 Herc.
22 Z Sgt.
S U. Maj.
S^2 Gem.
2 Y^2 Scor.
S Vul.
24 *l* Car.

November.

U Cas.
4 S^2 Cyg.
T For,
S Aq.
5 T Aqu.
V Vul.
6 V Oph.
8 R Vul.
S^2 Pup.
R Mic.
U Mon.
9 U Cet.
W Eri.
2 R C. Ven.
W Ori.
3 Y^2 Scor.
X Oph.
S Lib.
15 U Car.
16 W^2 Aur.
V Tau.
Y Per.
18 Z Hyd.
20 Y Cas.
U Vel.

Nove (con

d
20 Z A
21 X Li
V A
22 Z Cy
V Li
V D
T^2 C
23 S Le
S Mi
Z Au
25 R Sa
R D
26 S Bo
28 Z Pu
29 *l* Ca
30 S Pa
Z^2 S

Decem

1 S^2 Li
2 X Ca
S^2 Ce
X Ca
3 S Ta
Y^2 S
Z Ce
4 R Ly
5 T Ca
7 T Co
8 U C.
X Ce
R Ca
11 T H
12 S Ph
R Vi
T Ce
13 V V
W P
14 W O
16 R In
17 Y Ca
S Ca
18 T Ce
19 S^2 P
20 R Ph
R^2 S
22 X H
23 R Pi
Y^2 S
24 U Ca
U M
25 Z C.
30 S Vu
R Tr
31 X H

L. Min. = *Leo Minor.*   Mon. = Monoceros.   Pis. A. = Piscis Austral.   Sgt. = Sagittarii.

DOUBLE STARS.

No	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch. 1900+.	Observer
		h m	° ′		°	″		
1.	Σ 3062	0 0·9	+57 52	6½; 7½	348·1	1·48	4·00	Bk.
2.	OΣ 12, λ Cassiop.	0 26·4	+53 58	6½; 6½	153·8	0·50	3·96	Bk.
3.	β 395	0 32·2	−25 19	6·5; 6·7	293·6	0·57	2·80	D.
4.	β 495	0 43·4	+18 9	7½; 8	208·9	0·57	5·89	W.B.
5.	Σ 60, η Cassiop.	0 43·0	+57 17	3½; 7½	233·1	5·96	6·16	M.
6.	OΣ 20, 66 Piscium	0 49·2	+18 29	6 ; 6	315·0	0·43	3·54	W.B.
7.	Σ 73, 36 Andromedæ	0 49·6	+23 5	6 ; 6½	29·0	1·12	6·16	M.
8.	Σ 113, 42 Ceti	1 14·3	− 1 3	6½; 7	352·4	1·25	2·99	W.B.
9.	*h* 2036	1 15·0	−16 19	7 ; 7½	11·3	1·15	3·93	Bk.
10.	*p* Eridani	1 35·7	−56 42	6 ; 6	219·8	8·90	6·16	T.
11.	OΣ 38, γ Androm. BC.	1 57·8	+41 51	5½; 6	113·8	0·45	3·81	Ls.
12.	Σ 205, γ Androm. AB.	1 57·8	+41 51	2½; 5½	61·8	10·20	3·81	Ls.
13.	Σ 228	2 7·3	+56 59	6½; 7½	90·8	0·65	3·99	Bk.
14.	Σ 305	2 41·8	+18 58	7½; 8	311·8	3·00	5·04	W.B.
15.	Σ 333, ε Arietis	2 53·5	+20 56	5½; 6	198·0	1·11	5·05	W.B.
16.	β 525	2 53·5	+21 13	7½; 7½	129·0	0·28	3·33	Ls.
17.	Σ 367	3 8·9	+ 0 22	8 ; 8½	205·4	0·57	4·15	Bk.
18.	OΣ 53	3 11·3	+33 16	7 ; 8	228·4	0·64	3·20	Ls.
19.	Σ 425	3 33·8	+33 49	7½; 8	271·1	2·42	4·04	W.B.
20.	OΣ 531	4 0·8	+37 49	6½; 8½	134·4	1·69	3·20	Ls.
21.	OΣ 82	4 17·1	+14 49	7 ; 9	110·2	0·65	4·18	Bk.
22.	β 883, AB	4 45·6	+10 54	7½; 8	90·9	0·24	3·05	Ls.
23.	„ AC	......	......	7½; 10	157·6	17·60	3·06	Ls.
24.	Hussey 445	4 55·8	+20 41	8½; 8½	290·4	0·47	4·21	Ls.
25.	β 1238	4 55·2	+26 24	8 ; 11½	7·2	1·56	5·02	Ls.
26.	OΣ 98, ι Orionis	5 2·4	+ 8 23	6 ; 6½	169·5	0·92	4·10	Bk.
27.	Σ 742, 380 Tauri	5 30·4	+21 56	7 ; 7½	259·7	3·52	3·29	Bk.
28.	Σ 749	5 30·9	+26 53	6½; 7½	172·2	0·66	5·03	W.B.
29.	Σ 932	6 28·5	+14 49	8 ; 8	327·2	1·94	4·05	Bk.
30.	OΣ 149	6 30·0	+27 22	6½; 9	265·9	0·64	3·17	Ls.
31.	Sirius	6 40·6	−16 34	1 ; 10	112·9	5·82	4·19	Ls.
32.	OΣ 156	6 41·6	+18 18	6½; 7	298·3	0·50	3·12	W.B.
33.	Σ 1037	7 6·6	+27 24	7 ; 7	295·3	0·70	5·21	W.B.
34.	OΣ 170	7 12·2	+ 9 29	7 ; 7½	107·4	1·62	5·16	Ls.
35.	Σ 1066, δ Geminor.	7 14·1	+22 10	3 ; 8	207·3	6·72	5·18	W.B.
36.	β 579	7 27·9	+33 20	8 ; 10½	215·6	1·04	4·26	Bk.
37.	Σ 1110, Castor	7 28·2	+32 7	2 ; 3	223·8	5·55	4·16	S.
38.	Procyon	7 34·0	+ 5 29	1 ; 8½	5·3	4·46	5·17	Ls.
39.	Σ 1187	8 3·2	+32 31	7 ; 8	223·6	2·22	6·26	W.B.
40.	Σ 1196, ζ Cancri AB	8 6·4	+17 59	5½; 7½	346·1	1·16	6·14	M.
41.	„ „ AC	......	......	5½; 6½	108·7	5·13	6·29	M.
42.	„ „ BC	......	......	7½; 6½	121·1	6·13	5·23	W.B.
43.	Σ 1216	8 16·3	− 1 17	7½; 8	195·3	0·46	4·23	Bk.
44.	Hough 354	8 36·8	+26 24	8½; 9½	177·6	0·96	6·30	Ls.
45.	Σ 1273, ε Hydræ AB	......	......	4 ; 6	147·0	0·23	4·24	Ls.
46.	Σ 1273, ε Hydræ AC	8 41·5	+ 6 50	4 ; 7	232·2	3·51	4·24	Ls.
47.	Σ 3121	9 11·9	+29 0	7½; 7½	36·8	0·45	6·25	W.B.
48.	OΣ 201	9 18·0	+28 20	7 ; 11	223·6	1·66	5·17	W.B.
49.	Σ 1356, ω Leonis	9 23·1	+ 9 30	5½; 7	114·4	0·76	6·28	B.
50.	Σ 1389	9 45·3	+27 29	8 ; 9	307·3	2·30	5·19	W.B.
51.	A. C. 5, 8 Sextantis	9 47·5	− 7 37	5½; 6	85·1	0·49	4·17	Bk.
52.	OΣ 215, P. x. 23	10 10·9	+18 15	6½; 7	206·1	0·90	5·24	W.B.
53.	Σ 1424, γ Leonis	10 14·4	+20 22	2 ; 4	113·3	3·60	5·25	W.B.
54.	OΣ 218	10 22·1	+ 4 5	7 ; 9	75·5	1·09	3·29	Ls.
55.	OΣ 224	10 34·5	+ 9 22	7 ; 9	300·7	0·59	3·27	Ls.
56.	OΣ 229	10 42·3	+41 38	6 ; 7	319·3	0·88	3·40	Dob
57.	Σ 1523, ξ Ursæ Maj.	11 12·9	+32 6	4 ; 5	133·0	2·46	6·41	M.
58.	Σ 1536, ι Leonis	11 18·7	+11 5	4 ; 7½	51·9	2·39	6·26	B
59.	OΣ 235	11 26·7	+61 38	6 ; 7½	182·2	0·35	5·28	

DOUBLE STARS (*continued*).

No.	Star's name.	R.A. 1900.	Dec. 1900.	Mag.	Pos.-angle.	Distance.	Epoch 1900+.	Observer.
		h m	° ′		°	″		
60.	Lalande 21846	11 24·0	+31 1	7 ; 11	9·2	0·84	6·35	Ls.
61.	Σ 1647, 191 Virginis	12 25·5	+10 16	7½; 8	224·2	1·45	6·30	W.B.
62.	γ Centauri	12 36·0	−48 25	3·2 ; 3·2	352·9	1·52	5·47	T.
63.	Σ 1670, γ Virginis	12 36·7	− 0 54	3 ; 3	327·9	5·86	4·30	S.
64.	β Muscæ	12 40·2	−67 35	3·9 ; 4·2	345·0	1·33	5·73	T.
65.	Σ 1687, 35 Comæ	12 48·1	+21 48	5 ; 8	84·1	1·23	5·35	Ls.
66.	Hough 257	13 1·2	+26 46	8½; 9	154·6	1·75	6·30	W.B.
67.	OΣ 266	13 23·6	+16 14	7 ; 8	341·8	1·63	3·33	Ls.
68.	Hough 260	13 19·0	+29 45	8 ; 8½	321·2	0·70	6·37	W.B.
69.	Σ 1768, 25 Can. Ven.	13 33·2	+36 48	6 ; 8	133·4	1·11	4·42	W.B.
70.	Σ 1785	13 44·5	+27 29	7½; 8	300·0	1·51	5·28	Bk.
71.	β 801	13 40·7	+11 20	8 ; 10	326·5	2·63	4·46	W.B.
72.	Σ 1819	14 10·3	+ 3 36	7½; 7½	176·1	1·34	3·31	Dob.
73.	α Centauri	14 32·8	−60 25	1 ; 1	212·1	21·16	5·48	T.
74.	Σ 1865, ζ Boötis	14 36·3	+14 10	3½; 4	143·8	0·37	5·41	W.B.
75.	Σ 1877, ϵ Boötis	14 40·6	+27 30	3 ; 6½	327·0	2·60	5·63	M.
76.	Hough 263	14 43·3	+24 30	7 ; 10	175·4	1·07	6·47	W.B.
77.	Σ 1888, ξ Boötis	14 46·7	+19 31	5 ; 7	170·3	2·46	6·51	M.
78.	β 31	14 47·8	+19 9	8½; 9½	198·6	1·51	5·51	W.B.
79.	OΣ 287	14 47·9	+45 20	7½; 7½	322·5	0·71	3·35	Ls.
80.	Σ 1932, 1 Cor. Bor.	15 14·0	+27 13	6½; 7	152·9	0·48	5·35	W.B.
81.	Σ 1937, η Cor. Bor.	15 19·1	+30 40	6 ; 6	24·7	1·11	5·63	M.
82.	Σ 1938, μ^2 Boötis	15 20·7	+37 22	6½; 7½	66·7	1·03	5·60	M.
83.	OΣ 298	15 32·4	+40 8	7 ; 7½	193·0	1·25	6·67	Ls.
84.	Σ 1967, γ Cor. Bor.	15 38·5	+26 36	4 ; 7	118·0	0·72	5·36	Ls.
85.	OΣ 303	15 56·2	+13 33	7 ; 7½	145·0	0·74	6·44	W.B.
86.	Σ 1998, ξ Scorpii	15 58·8	−11 5	5 ; 5½	285·5	0·34	3·52	Bk.
87.	Σ 2032, σ Coronæ	16 11·1	+34 8	6 ; 6½	216·3	4·80	5·40	W.B.
88.	Σ 2055, λ Ophiuchi	16 25·8	+ 2 12	4½; 5½	60·5	1·18	5·47	Ls.
89.	Σ 2084. ζ Herculis	16 37·5	+31 47	3 ; 6½	175·4	1·09	6·55	Ls.
90.	De 15	16 41·5	+43 40	8 ; 8½	294·6	0·60	6·70	Ls.
91.	Σ 2140, α Herculis	17 10·1	+14 30	2½; 6	114·6	4·62	3·46	Bk.
92.	Hough 415	17 19·0	+25 51	8 ; 8½	329·3	1·30	6·55	W.B.
93.	Σ 2173, 221 Ophiuchi	17 25·3	− 0 59	6 ; 6	318·7	0·55	6·59	B.
94.	Σ 2262, τ Ophiuchi	17 57·6	− 8 11	5 ; 6	259·8	1·96	4·72	M.
95.	Σ 2272, 70 Ophiuchi	18 0·4	+ 2 32	4 ; 6	176·5	2·28	6·28	M.
96.	OΣ 358	18 31·3	+16 54	6½; 7	192·0	1·82	3·50	W.B.
97.	β 137	18 51·2	+37 15	8 ; 8½	133·1	1·35	4·74	Ls.
98.	γ Coronæ Aust.	19 0·0	−37 12	5½; 5½	123·5	1·69	5·65	T.
99.	Σ 2525	19 22·9	+27 8	7½; 7½	310·2	0·66	6·68	W.B.
100.	Σ 2579, δ Cygni	19 41·8	+44 53	3 ; 8	293·2	1·61	4·89	M.
101.	Σ 2695	20 27·7	+25 28	6 ; 8	76·4	1·05	4·54	W.B.
102.	β 151, β Delphini	20 32·7	+14 15	3½; 5½	100·3	0·15	6·79	B.
103.	OΣ 413, λ Cygni	20 43·8	+36 7	5 ; 6½	64·0	0·58	4·91	Ls.
104.	Σ 2799	21 24·0	+10 39	7 ; 7	298·0	1·66	6·84	Ls.
105.	Σ 2822, μ Cygni	21 39·7	+28 18	4 ; 5	124·0	2·22	6·62	W.B.
106.	Σ 2878	21 50·6	+29 3	9 ; 9½	354·6	3·47	4·87	Ls.
107.	β 172	22 19·0	− 5 21	6 ; 6	7·4	0·73	2·30	D.
108.	OΣ 483, 52 Pegasi	22 54·2	+11 12	6 ; 7½	224·7	1·05	2·90	Ls.
109.	β 79	23 12·5	− 2 4	8 ; 8½	81·1	0·91	3·89	Ls.
110.	β 720	23 29·0	+ 30 46	6 ; 6	349·0	0·34	3·95	Ls.
111.	β 858	23 36·3	+ 32 0	7½; 8	261·1	0·55	5·89	W.B.
112.	Σ 3050	23 54·6	+33 10	6 ; 6½	216·8	2·36	3·64	Dob.

In the column headed "Observer," the following abbreviations are used:—

Dob. Doberck.
Ls. Lewis.
B. Bryant.

M. Maw.
T. Tebbutt.

Bk. Biesbroeck.
W.B. Bowyer.

D. Doolittle.
S. Sternberg.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. JANUARY, 1907. No. 379.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1906 December 14.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder announced that seventy-seven presents had been received since the last Meeting, including, amongst others:—W. Bramsen, Japanese Chronological Tables, presented by E. B. Knobel; Optical Convention, Proceedings of the Meeting May–June, 1905, presented by the Committee; 'Oxford Astrographic Catalogue,' vol. i., presented by the University Observatory, Oxford; J. A. Pankhurst, 'Researches in Stellar Photometry,' presented by the Author; Royal Observatory, Greenwich, 'Astronomical Observations, &c.,' presented by the Observatory; Royal Observatory, Cape of Good Hope, 'Annals,' vol. xii. parts ii. and iii., and other volumes, presented by the Observatory. Astrographic Chart, 20 charts presented by the Royal Observatory, Greenwich, and 19 charts presented by the San Fernando Observatory; Photographs of the Spectrum of Mira Ceti, presented by the Rev. W. Sidgreaves.

Mr. W. W. Bryant, Mr. A. E. Conrady, and Mr. G. J. Newbegin were appointed Auditors of the Treasurer's Accounts for the year 1906.

Mr. Hinks gave an account of some of the results of his discussion of the photographic places of stars published in the Paris 'Eros Circulars.' A comparison *inter se* of the results from different observatories shows that the agreement is by no means so good as had been expected. Four series of observations are affected by systematic errors of one kind or another. The error in the Algiers plates is now well known; it takes the form of a magnitude equation, with a total range, both in R.A. and in Decl., of about a second of arc. The reason remains unknown. M. Trépied at one time thought that the object-glass was nipped in its cell; he had it readjusted, repeated some of the Eros fields, and kindly sent the measures to Cambridge, where they were reduced, with the result that the error appeared to be, if anything, slightly worse than before. A somewhat similar effect appears in the Toulouse declinations, which have a magnitude equation with a range of half a second of arc. It seems probable that in both these cases the error is in some unexplained way due to the object-glass.

The San Fernando results have also a magnitude equation in both co-ordinates, of similar range, but of somewhat different character—decidedly non-linear in R.A. These plates have been measured in one orientation only, which may account at least in part for the error. Finally, the plates which were taken at Catania and were measured and reduced at Paris, exhibit large discordances of a semi-systematic character, which do not depend upon magnitude, nor upon position of the star upon the plate, and whose origin remains obscure.

This investigation has been made in order to test the value of the various series of photographic results which will contribute to the determination of the Solar Parallax, and it appears that a considerable part of the published material is by no means fit as it stands for inclusion in a general discussion. This conclusion has an interest wider than that of the particular problem in hand. The Observatories chiefly concerned have shares in the Astrographic Catalogue, and have been working at it for many years. It is therefore surprising, and somewhat disquieting, to find in their Eros comparison star places errors much larger than are accounted tolerable in the Astrographic Catalogue.

Prof. Turner. I think all those taking part in the Astrographic Catalogue at any rate ought to be grateful to Mr. Hinks for the extraordinarily laborious series of researches he has conducted on these comparison stars. When the work for the Astrographic Catalogue was projected it was suggested that certain areas (Kapteyn's areas they were called) should be photographed by different observers in order that a comparison might be made. But it shows how little we foresaw the results of such a comparison that attention was principally concentrated upon the comparison of the *magnitudes* and not of the *positions*. No doubt we might have got some very useful results from the comparisons of magnitudes; but we have got first to attend to positions. By great good fortune, though very little has been done with Kapteyn's areas, this other piece of work has turned up and supplied a reason for this groundwork of comparison of plates which has fallen into the able hands of Mr. Hinks, and to those who are not familiar with such work I should just like to call attention to the enormous amount of labour these photographs and the few diagrams

Mr. Hinks put on the board represent. I can hardly give you any idea of it; but I can assure you it is very large.

The Astronomer Royal. I do not think I have any remarks to add to those of Prof. Turner, except to offer my congratulations to Mr. Hinks for the excellent work he is doing. I am sure we are all grateful to him for having brought these observations together and discussed them. The errors he has brought out are altogether beyond the range we should have expected. I do not know whether corrections have been made for errors of the réseau lines, but at any rate these discordances seem of an order different from any corrections which might be expected from that cause. I think they might be more likely due to a shift of the object-glass.

Mr. Hinks. The magnitude equations?

The Astronomer Royal. I was thinking of the later ones of Catania.

Dr. Rambaut. As is no doubt the case with other Fellows, I have listened with great interest to Mr. Hinks, and look forward to studying his paper in more detail in the 'Monthly Notices.' Meanwhile, I would like to ask him a question. In the course of his remarks he thought these errors might be attributed to maladjustment of the object-glass. Can he give us a hint as to how such mal-adjustment led to these errors? I am not questioning the value of the suggestion, but I find a little difficulty in conceiving how such a thing could come about.

Prof. Dyson. Though I missed, I am sorry to say, what Mr. Hinks has said, I have seen the proof of his paper. One of the points he referred to well deserves attention—that is, what effect there can be in the reversal of the object-glass. I notice that he found there was nothing of the nature of magnitude equation in the visual observations of declination; and that, too, seemed to be an important result to have obtained. If I might make a suggestion to Dr. Rambaut I would say: Is it not possible that a tilt of one of the two lenses of the object-glass to the other might make an unsymmetrical image which would give a different nucleus for bright stars and for faint stars?

Prof. Turner. I suggested tilting and the Astronomer Royal suggested shifting.

Mr. A. E. Conrady. There is one explanation of magnitude equation in declination which Dr. Pulfrich discovered in working with the stereo-comparator which might possibly account for some of these results. Dr. Pulfrich found that photographs of the same region taken at different times—and apparently the length of interval had nothing to do with it—had a distinct stereoscopic effect between the fainter and the brighter stars. With half-yearly intervals parallax would have suggested itself as an explanation, or, in long intervals, proper motion. But this happened sometimes with photographs taken the same night. It was eventually found that the phenomenon occurred when one photograph was taken on one side of the meridian, and the other on the other side of the meridian, and that it appeared to be due to the dispersion of the atmosphere, which drew the star-image out into a short spectrum; and it is well known that in a faint spectrum the centre of gravity, so to speak, of the light lies at a different wave-length from its position in a bright spectrum. I think Dr. Pulfrich made out a good case, and that possibly the dispersion of the atmosphere may account for some of these magnitude equations.

Mr. Inwards. It struck me in reading this paper that it might possibly be due to the hygroscopic condition of the gelatine of the photographs. And it must also be remembered that even a small pit of dust on one part of the plate would make a difference. These are only suggestions from the photographic point of view.

Mr. Henkel. One remarkable point I noticed in the account given by Mr. Hinks, that the errors in declination in one case were so curiously opposite to one another that they would almost balance: it seems to me as if these are due to some optical effect.

The President. The difficulty is to reconcile these various explanations with the fact that on the same plate you get images corresponding to stars of different magnitude affected differently. If the shift of the images is due to hygroscopic action on the film, all images in a certain part of the plate should be affected similarly. On the other hand, if the effect is due to the action of the object-glass being such as to produce an irregular spreading of the image of a bright star during exposure, then the image of a bright star taken with a short exposure should have a different position to the image obtained with a long exposure. I do not know if any effect of this kind has been noted. The other explanation which has been suggested, namely, that the effect of refraction may be different for a small star to what it is for a brighter star, does not seem to explain the differences in right ascension, for in that case the errors in the plates taken on the west of the meridian should be all one way, and those in plates taken to the east of the meridian should be the other way. I do not know whether or not Mr. Hinks in his reply will be able to reconcile these difficulties.

Mr. Hinks. I am very grateful for the various kind things said about this work. With regard to the particular points raised in the discussion: Dr. Rambaut asks what kind of error in the object-glass would produce magnitude equation. I am afraid I cannot furnish definite observations; but it seems to me that almost anything unsymmetrical in the object-glass would produce magnitude equation. I suppose that increase in size of the image is produced, at least in part, by the buzzing about of the first diffraction ring, and that if dissymmetry of the objective made that ring brighter in some parts than in others, we should necessarily have magnitude equation.

Dr. Rambaut. Would that be independent of the position of the star on the plate?

Mr. Hinks. The main effect does not seem to depend very much upon the position of the star, except perhaps at Algiers. But there is difficulty in discussing this point, because the bulk of the faint stars measured are confined to a central square of twenty minutes of arc.

Prof. Dyson. May I ask how far you go from the centre of the plate?

Mr. Hinks. These results refer to all the published places. In many cases they would go right up to the edge of the 2° 10′ plate.

Then with reference to Mr. Conrady's remarks, may I ask where Dr. Pulfrich's paper was published?

Mr. Conrady. It appeared in the 'Zeitschrift für Instrumentenkunde,' in German, about four years ago.

Mr. Hinks. It seems to me that there is a possibility of atmospheric dispersion giving magnitude equation in declination, but not in right ascension.

Mr. Conrady. He used plates oriented north and south, and only the observed displacements of the faint stars were in right ascension.

Mr. Hinks. I see we are thinking of two different things; between pairs of plates taken on opposite sides of the meridian the differential effect will be in right ascension; in the mean of a great number of plates equally distributed the absolute effect will be almost entirely in declination.

With regard to Mr. Inwards's remarks on hygroscopic effects and tilt, the *réseau* would eliminate the former, and the tilt of the plates is rigorously controlled, so that I do not think it can have any sensible effect.

With reference to the question of dispersion raised by Mr. Maw, I do not think that atmospheric dispersion can be responsible in any way for the magnitude equations which we have been discussing, which appear as systematic discordances between observations nearly in the same latitude, and not as an absolute error affecting the general mean of all.

Mr. Conrady. Does not dispersion necessarily accompany the atmospheric refraction? The dispersive power of the atmosphere is, I believe, on about the same scale as that of glass. Therefore a refraction of 60 seconds would have a dispersion of about 1 second going with it.

The President asked Prof. Turner to address the Meeting on the subject of the Volume of the Oxford Astrographic Catalogue which had been presented to the Society.

Prof. Turner. Vol. I., as you see, of the Astrographic Catalogue is complete and Volume II. nearly complete. There will be 8 volumes altogether, 7 volumes similar to this, and the 8th will contain the discussions. We are accordingly by no means at the end of our work, for the actual proof-reading of these volumes is a very serious task for a small Observatory. Three of us have been engaged upon it—Mr. Plummer, Mr. Bellamy, and myself. Mr. Bellamy has, of course, done the lion's share, but we have all three read about a million figures this year in proof. Three more years must elapse before we can hope to finish the printing; but the actual work of the measurement is, I am glad to say, done; it was done some two years ago. It may not be out of place to remind this Society that this work was begun at a Conference called in the spring of 1887, which will soon be twenty years ago, yet none have finished even the minor enterprise of cataloguing, and some Observatories have not even begun to measure. But we may hope that some advance may be made now that Sir David Gill is back in England with nothing to do, so to speak; may we hope that he will induce the Conference to take some kind of action? I am afraid I occupy one extreme wing of the party on the question as to the time to be allotted to the work. I should like to see it done in the very minimum of time; where others seem equally interested in stretching it out, of course in the interest of accuracy, to the maximum of time. But it seems to me there are occasions when something has to be sacrificed, and I hope this time it will not be the completion of the work, even if we lose a little accuracy. For, consider how important it is to carry out an enterprise of this kind within some period which shall be comparable to the life of a human being, or at any rate the active life of a Director. And consider, on the other hand, how many of the Directors of the Observatories which started on this enterprise are still in active work. Very few, three or four in all. Paris has changed twice, for instance. Sir David Gill himself is on the point of resigning. But I need not go through the list. That, I think, is one of the reasons for making our scheme a little more modest in the immediate future. Another point to which I have repeatedly called attention is the necessity of considering the great diversity in the financial conditions of the participating Observatories. There are Observatories, especially in America, where money is freely forthcoming, and in those places we find quite rightly—and, I trust, I at any rate have not shown any want of sympathy,—we find great enterprises involving almost reckless expenditure, which, I think, is thoroughly in the interest of astronomy. On the other hand, there are places—I think our University Observatory is at the other extreme—where the money available for learning is already assigned. It is not as in America, where they are only beginning to wake up to the fact that there are enterprises of this kind, and thus they afford plenty of money to carry them out: with us there are old-established vested interests, so that every penny must be scrutinised most carefully before being spent. I think, then, the spirit in which an enterprise can be attacked in places of such diverse character is totally different, and when formulating a scheme to include them all we should pay attention to the weakest link rather than the strongest.

Well, there has been great difficulty in getting the money at Oxford: but this difficulty has led to at least one very pleasant incident. The money for the telescope with which the work is done was furnished by the generosity of our dear friend Dr. De la Rue, the well-known Fellow of this Society; and in this connection I cannot help reflecting on the large consequences which sometimes spring from our friendly meetings in this room.

Another difficulty was to get the money for the printing; and here I might be allowed for a moment to defend myself against the charge of not looking ahead. I began trying to get funds for the printing immediately the end of the work was sufficiently well in sight to be able to use that as a consideration. But it took more than two years to get the funds, which were ultimately obtained through the friendly intercession of the Royal Society. Hence arose a difficulty I had not quite foreseen, which was that our staff that measured the plates was necessarily disbanded because the money for measuring had come to an end, and we had to start proof-reading with entirely new people (with the exception of Mr. Bellamy, Mr. Plummer, and myself), so that the difficulties of not getting money affected us in a variety of rather unexpected ways. It will not be surprising, therefore, that a good deal of stress has been laid on economy throughout. As an example I may instance the retaining of only three figures in the measurements, which I think at any rate has saved us some years of work. We might have done more accurate work, but it seemed necessary to make that decision. I think that the full study of accurate methods will only come after we have completed our work. In some ways it would be better to risk doing the work slightly wrong in the first instance, if thereby we can get it done. I know I am very heretical here. We can at any rate study the errors, and then perhaps do it again, because, for one thing, I do attach great importance to getting the work *done again* to get the proper motions. As an instance of what can be done when a single volume is complete, I would like to draw attention to some of the results which come from merely counting the number of stars in different parts of the plate. When the volume is once in print that is a work easily carried out. It has been in the present instance undertaken very kindly by a lady who wishes to remain anonymous. She divided the plate into four quarters; the number of stars in various circles at different distances from the centre was counted, each quarter separately; and the totals were divided by the number of squares, so as to get the relative density practically along four radii.

But before coming to this distribution in detail let us first consider the total number of stars in each quarter-plate, which were found to be as follows:—

(N.W.) 16,491	17,142 (N.E.).
(S.W.) 15,349	16,720 (S.E.).

It will be seen that there are over 10 per cent. more stars in the N.E. quadrant than in the S.W., the other quadrants being intermediate. What possible causes are there for such a discrepancy? It cannot be due to the stars themselves, at any rate as regards the difference in R.A.; for on adding up all the plates round a complete circuit, such an effect would be eliminated. A second possible cause is tilt of the plate. It may be that if one part of the plate is nearer the object-glass than another it is better focussed and shows more stars. But now I will exhibit the distribution of density along the four radii, which will show that the effect cannot be (at any rate wholly) due to tilt, but must be in part due to the behaviour of the object-glass, and thus comes into the class of errors of which Mr. Hinks has found quite independent evidence. First, let us recall some facts about the star-density at different distances from the centre which were laid before the Society some years ago. It was shown that the density was not greatest at the centre but at some distance from it for which the plate had been focussed, falling off both within and without this distance; and the inference seems to be that the surface of best focus is curved and cuts the flat plate in a ring, the radius of the ring increasing as the plate is pushed in towards the O.G. If, then, the plate is tilted, *i. e.* is pushed further in on one side (say the N.E.) than on the other (say S.W.), the maximum density will be further from the centre in the N.E. quadrant; but we should not expect it to be numerically greater than the S.W. maximum. Now, from the density distribution given below, it will be seen that the N.E. maximum is not only further out, but much greater than the S.W. maximum; and hence I think the discrepancy must be, in part at least, due to the O.G. [I can only as yet give the figures for the first six hours of R.A., the work for the others not being quite completed.]

Distance from centre ..	4′	13′	22′	30′	39′	48′	56′	65′	74′	82′
Density N.E. quarter ..	15	14	16	17	22	25	24	25	20	18
Density S.W. quarter ..	13	16	16	20	19	20	19	16	13	10

The investigation is still to be completed; but the point seems worth the attention of those who have measured a number of plates, and can therefore test the performance of their object-glasses in this way.

There is another quite different point on which I have submitted a note to the Society, hoping that it may save the time of other workers: a point that arises in the reductions. The meridian places available for plate-constants are twenty years old, and are thus affected by unknown proper motions. Near the Milky Way there are so many "known" stars that the errors of the places balance one another, and we get very fair plate-constants; but where the stars are few this is not so. The deduced plate-constants are sensibly in error, as is shown by the fact that different residuals are obtained for the individual stars from different (overlapping) plates. In regions near the Milky Way these residuals are accordant; but in thinly-starred regions we get differences of 2″·5 or 3″·0 between the places found for the same star from different plates. We have spent much time trying to find mistakes or to improve the plate-constants; and we regard this now as time wasted, for it is shown in the note referred to that unless the meridian places are corrected so as to satisfy a certain condition we are bound to have such discrepancies. The condition is that what I have called the "inconsistency" of each plate should be in some way reduced to zero, otherwise the adopted places cannot be fitted by any linear solution. This condition may possibly be secured by a series of approximations—taking the means of those found in the first solution, correcting the places accordingly, and then solving again; then repeating the process; or by tabulating the "inconsistency" of each plate we may reach the result of such a series of approximations by a compendious process. Or, again, from a study of these "inconsistencies" we can see where improved meridian places are most needed, and obtain them in such cases. In other words, we need not re-observe the whole of the stars, but we might get improved places for some. And that brings me back to the question as to whether the re-observation of the whole of the stars for the Astrographic Catalogue is a thing that is possible; it is part of the general question as to how big a programme we can undertake during the next ten years. I sincerely hope it will be possible; that would put an end to the difficulty; we should have improved places which would satisfy the requisite conditions.

But *meanwhile* what are we to do? The number of zones which will have improved places of stars from meridian observations is comparatively small even in the Northern hemisphere, and unless we can find some way of getting corrections of a more or less satisfactory kind the measures of a great part of this work will be used at a disadvantage. The general moral of my remarks is that I hope we shall have at an early date a Conference which shall meet with the determination to see this work finished within a limited time.

The Astronomer Royal. I would like to offer my congratulations to Prof. Turner on having brought out the first volume of the Astrographic Chart. I need hardly say I have been extremely interested and glad to see how excellently the work has been done. With reference to his point about reference-stars, I may say that the re-observation of these stars has been already begun at Greenwich and that we hope to complete it within the limits Prof. Turner has set us, that is in nine years, and then there will be good data for the determination of proper motions, which will be a valuable thing; and also for correction of the places of the stars to be used for deducing the plate constants of the Astrographic Catalogue. But, of course, the difficulty at present is to deal with the existing state of things being dependent upon places of reference-stars observed a certain number of years ago.

Mr. Franks. I would like to ask Prof. Turner one question. Are there not some errors due to curvature of the field? If we focus for the centre the plate is out of focus at the edge and *vice versa.* The usual plan is to focus for an intermediate place. Then if the plate is slightly tilted too no doubt there would be more stars on one part than on another.

Prof. Dyson. I feel strongly about the importance of meridian re-observation of the whole heavens as reference-points for the Astrographic Catalogue. And I do not think it is such a very arduous work. You see in ten years the Astronomer Royal has re-observed some 10,000 new stars and is now re-observing another 10,000; and Sir David Gill has observed some 10,000 stars; and the whole sky would require from 120 to 150 thousand reference-stars. It is a quite possible programme if its importance is realized and if people combine. I quite think Prof. Turner may be possible to improve the results to some extent by the consideration of overlapping plates, but there are some errors which I think cannot be eliminated in that way. In 25 years there has been a fairly considerable movement of the stars in any particular part of the sky, due to what used to be considered as the effect of solar motion; and this is of a systematic character which no comparison of adjacent plates can correct. I think we may congratulate ourselves that our English astronomers have realized the importance of re-observing the reference-stars. Others will have to face the same problem.

Dr. Rambaut. I should like to ask Prof. Turner whether he has investigated the effect of inclination of the plate. From a somewhat hasty perusal of the catalogue which had been kindly sent me I think he has used linear transformations for deducing the standard co-ordinates. Would not the effect of such inclination be to change the number of stars in the quadrants as represented in that diagram?

Prof. Turner. I think that what Dr. Rambaut is saying is equivalent to asking is there any tilt in the plate. I hope there is not; in any case I tried to show why it should not be tilt entirely, by considerations adduced from the position of the maximum. But, Mr. President, I am glad of this opportunity of saying something more, because I did not say what I intended to do about the devotion Mr. Bellamy has shown to this work. Though he was not present at some preliminary work (when Prof. Pritchard and the Astronomer Royal were in correspondence about the object-glass) yet he started soon after that and has taken all the plates practically and superintended the whole work. I have said that the work would probably not have been done without the help of De la Rue: certainly it would not have been done without Mr. Bellamy.

The President. I am sure that in thanking Prof. Turner for his communication you will desire also to congratulate him on the progress of his work, and—after the statement he has just made—you will wish to add your congratulations to Mr. Bellamy.

Mr. H. F. Newall then gave the abstract of his paper on "Spectroscopic Observations of the Sun." The observations were made at first with the 25-inch equatorial (the Newall Telescope), and later with a fixed horizontal telescope, a coelostat, and auxiliary mirror: the latter equipment appeared to possess considerable advantages. An account was given of the preliminary experiments, the experimental arrangements were described, and some results were given of the photographic study of the bands and flutings in the spectra of sun-spots.

The President. We feel that Mr. Newall is such an earnest worker that everything he does deserves our careful attention. We are glad he has been able to undertake this particular branch of research at Cambridge as so much is being done at Mount Wilson under very favourable conditions that we are apt to be disheartened in this country as to our position in that particular branch of astronomical work. Thanks to what is being done by Mr. Newall at Cambridge and by Dr. Lockyer and Mr. Fowler at Kensington, we feel that we shall have a number of important facts to add to the general store of knowledge of solar physics.

Mr. Fowler. I should like to express my hearty congratulations to Mr. Newall, not only on the excellent results which he has already obtained, but also on the splendid promise for the future. I know from experience the difficulty of mapping the sun-spot spectrum from visual observations, and it will be a tremendous advance when the photographic work relieves us from this laborious task. I was particularly sorry to see Mr. Newall turn over so many pages of his paper without telling us what was on them, but look forward with all the greater interest to seeing the details in print. There are several points one would like to ask more about, but as we are so near the end of the Meeting I think it better to refrain from doing so.

Mr. A. C. Crommelin presented a paper on the approaching return of Halley's Comet. He called attention to a paper by Dr. A. Angström, published in 1862, in which a mean period for the comet of 76 93 years was deduced with inequalities due to the action of Jupiter and Saturn. Dr. Angström's results give 1913·08 for the time of the next perihelion passage, while that given by the Count de Pontécoulant is 1910·37. In view of this large discrepancy of 2·7 years, the speaker thought it most desirable that the perturbations should be independently computed. Before the last return in 1835 there were at least five independent determinations of the orbit, and it might reasonably be hoped that at this return as great success might be achieved in the prediction as in 1759 and 1835.

Photographs of the spectra of Mira Ceti taken at Stonyhurst in 1897 and 1906 by Father Sidgreaves were shown. Attention was called to a great increase of brightness of the Hβ line in the recent spectrograms.

Dr. Lockyer also showed photographs of Mira Ceti taken at the present maximum.

The following papers were announced and partly read:—

Royal Observatory, Greenwich. "Observations of Comet *e* 1905 and Comets *a* and *b* 1906, from Photographs taken with the 30-inch Reflector of the Thompson Equatorial."

"Pogson's Observations of U Geminorum," edited by *H. H. Turner*.

P. H. Cowell. "Hansteen's Eclipse at Stiklestad, 1030 August 31."

A. C. D. Crommelin. "The Proper Motion of Castor."

W. G. Thackeray. "Note on some Proper Motions derived from a Comparison of Carrington's Catalogue, 1855, with the Greenwich Places for 1900."

A. C. D. Crommelin. "Note on the approaching Return of Halley's Comet."

W. M. Witchell. "On the Accidental Production of Temporary Errors of Division on a Graduated Circle."

A. Fowler. "Note on Silicon in the Chromosphere."

A. Fowler. "The Enhanced Lines of Iron in the Region C to F."

Royal Observatory, Greenwich. "Observations of Minor Planets from Photographs taken with the 30-inch Reflector of the Thompson Equatorial."

H. F. Newall. "Notes on some Spectroscopic Observations of the Sun."

W. G. Thackeray. "Estimate of the Number of Stars within certain Limits of Proper Motion."

The following gentleman was elected a Fellow of the Society:—

Major Alexander Davidson Fleming, Artillery Mansions, 75 Victoria Street, London, S.W.

The following Candidates were proposed for election as Fellows of the Society:—

Edgar T. Adams, 5 Warkworth Street, Cambridge (proposed by E. T. Whittaker).

Robert Jonckheere, Observatoire Stella, Roubaix, France (proposed by Camille Flammarion).

John Stewart, Chief Officer, R.M.S. 'Empress of China,' The Willows, Wallasey, Birkenhead (proposed by E. B. Knobel).

Samuel Veevers, Normanton, Kimberley Drive, Great Crosby, near Liverpool (proposed by R. C. Johnson).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1906 November 28.

F. W. Levander, *President*, in the Chair.

Secretary: J. G. Petrie.

Mr. Petrie read the Minutes of the previous Meeting, which were confirmed and signed. He then read the list of presents received, and the thanks of the Association were accorded to the respective donors.

The names of 6 Candidates for Membership were read and passed for suspension, and the election by the Council of 9 new Members was unanimously confirmed.

The President, after thanking the Council for the honour they had conferred upon him in electing him as their President, announced that the Council had decided that a series of short addresses or lectures should be delivered at some of the meetings of the Association. These would not, however, be of such a length as to cut very short the time for papers and discussion thereon. The first of the series would be delivered by Mr. E. W. Maunder at the Meeting on December 19. The subjects of the lectures would be announced in the Journal.

Mr. E. Holmes read a paper on "The Shadow-Bands." He said that in dealing with the question he laboured under the disadvantage of never having seen an eclipse, but this, at any rate, enabled him to compare the observations of others the more impartially. The first thing that struck him when reading the observations of the shadow-bands was their very indefinite character, their variableness, and their many apparent contradictions. In fact, there was hardly a detail of the data but what they had apparently contradictory testimony in relation to it. Miss Stevens appeared to have made the most careful and systematic study of the phenomenon, and her conclusion was that the shadow-bands were a purely local atmospheric phenomenon, and in their origin had no element of connection with the peculiar circumstances of an eclipse. Mr. Holmes proceeded to argue that the balance of evidence on the whole did not confirm this view, and was more in accordance with the theory that the bands were diffraction-effects, and he quoted Dr. Barnard's opinion that the diffraction of the Sun's light ought to produce sensible fringes in the few moments just preceding total obscuration by the Moon. Many of the discrepancies of the observations were reconcilable on that supposition; those who saw the bands 1 inch apart and those who saw them 10 inches or more would be equally correct if a short interval elapsed betwixt the observations, and the speed of movement at different stations would obviously be different, according to their positions relatively to the advancing shadow. It appeared to him that the speed of the bands would be neither that of the Moon in her orbit nor of the edge of the shadow, but rather that of a closing slit, and that the motion would be the reverse of that, or towards the Sun, both before and after the eclipse.

Miss C. O. Stevens described the arrangements she had made for observing the shadow-bands, and she was satisfied that the results confirmed her theory that shadow-bands were but the projected images of ripplings in the Earth's atmosphere, due not to the elements of an eclipse at all, but to the prevailing elements of movements in the Earth's atmosphere, and made visible by the special conditions of illumination. The ripplings round the Sun's limb varied enormously in amplitude, and such great variation might well account for the fact that different observers at different stations recorded such great variations in the width of the dark shadow-bands and their intervals apart. She added that shadow-bands had been seen at other times than at times of eclipse.

Capt. Alfred Carpenter, R.N., said he could confirm what Miss Stevens had said about the ripplings. She was perfectly correct in saying that the main movement of the upper atmosphere determined the movement of the ripplings on either side of the Sun. In the two eclipses he had been able to observe in Spain, the observers making it part of their work to observe shadow-bands never disagreed in the width of the bands.

Mr. G. M. Seabroke said it seemed to him the question was as between whether it was an atmospheric ripple or a diffraction phenomenon. If it were a diffraction phenomenon the direction of the bands in the motion must be the same in each eclipse with reference to the position-angle of the crescent of the Sun. If, on the other hand, it was a purely atmospheric phenomenon, one would expect it to vary in respect to different atmospheric conditions quite irrespective of the position of the crescent. It seemed, at first sight at any rate, that diffraction would sufficiently account for it if the direction of the shadow-bands were such as should be expected from the diffraction phenomenon.

Mr. Crommelin said that ever since the diffraction hypothesis was put forward, which was many years ago, he had felt that the velocity of these bands was incompatible with it. The very least speed of the shadow on the Earth's surface was 24 miles a minute, and he thought that they would all agree that no one could possibly see any details of bands moving over a sheet at that pace. His own idea was that, as Miss Stevens had said, they were ripples in the atmosphere; but he thought that the disappearing crescent of the Sun did play an important part, he also thought that the lowering of temperature that took place also played a part. That, probably, made the ripples more or less parallel to the direction of the advancing shadow, and so in a position for the disappearing slit of the Sun to make them effectively visible on the ground.

Mr. E. W. Maunder said that the late Dr. Copeland had observed shadow-bands on the inner walls of the great dome of the observatory at Dun Echt. The Hill of Fare, 5 miles off, cut off the Sun little by little, so as to reproduce to some extent the conditions of an eclipse, and shadow-bands were distinctly seen on several occasions.

Mr. S. A. Saunder was afraid that the absence of colour and the number of bands were absolutely fatal to any diffraction theory. Then again, with regard to the ripplings, Mr. Holmes had suggested that they were due to clouds, and had pointed out that the bands were seen when there were no clouds in the atmosphere, but he (the speaker) thought they were not due to clouds at all, but were produced by the crossing of two currents of air.

The President said that Mr. Holmes's paper had led to a very interesting discussion. He thought Miss Stevens's suggestion was a good one, and if all the various accounts of shadow-band observations could be brought together a satisfactory theory might be formulated.

Mr. A. Hutt read a paper on "Sun-spots and Thunderstorms." Mr. Hutt said he had been investigating the causes of these and kindred phenomena for many years, and had arrived at the altogether unlooked for result of a similar cause for sun-spots and thunderstorms. He then submitted a table showing their agreement for the year 1903.

Mr. F. W. Longbottom said they had been passing through a period of sun-spot maximum and had had singularly little thunder. This would require a good deal of explanation on Mr. Hutt's theory.

Mr. Holmes asked whether it was only thunderstorms that took place in London that affected, or were affected by, sun-spots. Probably never a day passed but there was a thunderstorm somewhere in the world.

Mr. Maunder pointed out that Mr. Hutt's figures did not advance the matter. There was hardly a single day in the year 1903 in which there was not either a fresh sun-spot or a violent and conspicuous change in one, so that a thunderstorm, happen when it might, could not fail to be coincident with one.

Mr. Maunder then threw on the screen some slides sent by Mr. Hardcastle, amusingly illustrating the misuse of coincident statistics. He also exhibited some photographs, from Greenwich, of some of the more important spots of recent times. He remarked that his own experience had been that whenever we had remarkable weather of any kind—cold, hot, dry, or wet—at once a number of people said it was due to sun-spots. It did not matter in the least whether there were many or few sun-spots at the time.

An interim report of the Solar Section, devoted principally to spectroscopic observations by Capt. Daunt and Mr. A. A. Buss, was then read.

The President said they ought certainly to congratulate both

Capt. Daunt and Mr. Buss on the excellence of the work they were doing with comparatively small instrumental means.

Mr. Maunder and Mr. Crommelin gave some interesting particulars as to the tracks of the eclipse of January 14th next and the eclipses of 1909, 1912, 1914, and 1927, illustrated by lantern-slides.

The President announced that Mr. Wilding had been obliged to retire from the Directorship of the Photographic Section, and that Mr. F. W. Longbottom had kindly undertaken the Directorship of the Section.

Mr. F. W. Longbottom believed that the Section might be made a very useful one, and that it would do valuable astronomical work. The question of illustrating the Journal more frequently had been suggested by the late President, and that was a very desirable feature. He wished the Members of the Section to look upon him not so much as their Director as their rallying point—their Secretary.

The President thanked Mr. Longbottom for acceding to the request that he would preside over the Section, and expressed the hope that in that position he would have plenty of work to do.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The Monthly Meeting of this Society was held on Wednesday evening, December 19, at the Institution of Civil Engineers, Great George Street, Westminster, *Mr. Richard Bentley*, F.S.A., President, in the Chair.

Admiral J. P. Maclear read a paper on "The Guildford Storm of August 2nd, 1906." This storm shows some very curious and interesting features, in the remarkable violence of the wind, rain, and hail, within a small area, and the suddenness with which it burst. There was an area of thunderstorms over the whole of the south of England on the evening of that day. The most violent storm, however, burst over Grayshott on Hindhead at 8.20, and pursued a narrow track through Godalming and Guildford to Ripley, 5 miles north-east of Guildford. The wind was of hurricane force and blew down an immense number of trees and caused other damage and also the loss of two lives. The rain, accompanied by large hailstones, was very heavy, as much as 1·17 inch falling at Grayshott in fifteen minutes. There was a magnificent display of lightning, the flashes being incessant and darting from cloud to cloud.

Mr. R. Inwards read a paper on "The Metric System in Meteorology." The author did not discuss the general question of the advantages of the metric system over that in use by Britain and her colonies and United States of America, but confined his remarks to the advisability of adopting some uniform system for all the meteorological observers upon the globe.

Sun-spot Spectra *.

THE photographic investigation of sun-spot spectra was set on foot at the great Solar Observatory directed by Prof. Hale in August 1905. The instrument employed was the horizontal reflector of 24 inches aperture and 60 feet focus, known as the "Snow Telescope," in conjunction with a "Littrow" spectrograph of 18 feet focus, light being supplied by a coelostat mirror 30 inches in diameter. The practically useful range of sensitiveness of most of the plates exposed with this powerful apparatus was from D to Hβ; but there were available besides some spectrograms of large spots, recently taken by Mr. Ellerman, extending from the extreme red into the ultra-violet. Laboratory work was simultaneously carried on, latterly with the aid of an electric furnace built at Pasadena; so that first-hand interpretation of the recorded phenomena proceeded *pari passu* with their observation. The treatment of the difficult subject taken in hand was thus singularly complete, and it yielded results of commensurate importance. They may be briefly summarized under two headings:—

First, the production of cumulative evidence to the effect that the temperature of spot-umbræ is lower, not only than that of the photosphere, but also than that of the reversing layer.

Secondly, the establishment of an unmistakable analogy between the spectra of sun-spots and of red stars, emphatically confirmed by the recognition, among the elements of spot-absorption, of several of the titanium-flutings characteristic of the "Antarian" stellar variety.

Let us recall the nature of the appearances that have to be studied. We view sun-spots through an immense depth of enveloping strata, any or all of which may partake of the disturbance occasioning the spot. Umbral and penumbral light, accordingly, is liable to be modified anywhere along the line of sight until it is clear of the corona. Hence the abnormal spectral traits, which seem to characterize spots, need to be carefully localized. Some, no doubt, are produced in the vaporous sink of the umbra, but others quite possibly originate high up in the chromosphere. It is not invariably easy to discriminate. The darkening of spots is due, in the main, to a dusky band running from end to end of the spectrum, but gaining intensity towards the blue end. It is at times partly resolvable into a multitude of closely adjacent, fine lines, with here and there a chink through which photospheric radiance shines almost undimmed. This quasi-continuous absorption is clearly unconnected with the "smoke-veil," by which sunlight obtains its mellow tinge. It is likewise entirely distinct from the spot-bands, a regular series of which was noted by Mr. Maunder between *b* and E, 1880–6, while a less refrangible set came into view at Stonyhurst *. They remain unexplained, but have proved to be recurrent. The most obviously distinctive feature of sun-spot spectra, however, is the strengthening in them of a considerable number of the Fraunhofer lines. The selection is not a little remarkable. A large majority of the emphasized lines are due to iron, chromium, manganese, titanium, and vanadium. Now the atomic weights of these five metals are not widely different; and the highest of them—that of iron—is 56. Hence, the constipated vapours in spot-cavities are not chosen out for their density: the chemistry of these strange formations is not determined by mere settling down; if it were, the heaviest metals, such as mercury, lead, gold, platinum, should be prominent, and there is absolutely no sign of their presence. The indications of spot-spectra, in fact, refer, not to the nature of the substances impressing upon them the stamp of their absorption, but to the conditions of their incandescence.

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* 'Preliminary Paper on the Cause of the Characteristic Phenomena of Sun-spot Spectra.' By George E. Hale, Walter S. Adams, and Henry G. Gale. (Carnegie Institution of Washington, 1906.)

The most essential of these conditions is, in the opinion of Prof. Hale and his coadjutors, a lowering of temperature. They point out that the strengthened lines occur exclusively in the visible part of the spectrum, and predominantly in the red, yellow, and green, connecting this symptom with the general experience of an increase in the relative intensity of the less refrangible lines in a given spectrum at a lowered heat-leavel. Further, they have ascertained that over ninety per cent. of the lines tabulated as strengthened in sun-spots are also strengthened in passing from a thirty-ampère arc to a two-ampère arc. The probability is certainly strong that the temperature of a weak arc is lower than that of one much stronger; and if so, the case is proved. Still, there is room for controversy. But this item of evidence does not stand alone. Spot-bands, since they presumably originate from chemical compounds, give concordant testimony; and the recent detection of titanium-oxide flutings as contributory to spot-absorption leaves small doubt on the subject. They were vainly sought by Mr. Fowler in 1905 †; he employed visual means, and they come out distinctly only upon sensitive plates. Their identification on Mount Wilson places beyond the reach of cavil the affinity of third type stars to sun-spots. Already in 1875, Father Secchi had an inkling of the fact; Sir Norman Lockyer, on express grounds, adverted to it later; and Professor Hale derived from his researches on fourth-type stars at the Yerkes Observatory a provisional certainty that the metallic lines characteristic of both the "banded" varieties of stellar spectra agreed in the main with the thickened and darkened lines in sun-spots. Special attention was accordingly given to the point on Mount Wilson ‡, with the upshot of establishing a stellar sequence represented by the Sun, Arcturus, and Betelgeux, the middle term of which showed nearly the same

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* *Observatory*, vol. ix. p. 373.

† Transactions of the International Union for Solar Research, vol. i. p. 217.

‡ Hale and Adams, *Astroph. Journal*, vol. xxiii. p. 400.

kind and amount of deviation from the solar pattern visible in spots, while the third member of the series (Betelgeux) seemed to have advanced further in an identical direction. "This sequence," Mr. Adams remarks, "would represent a scale of descending temperature" if the explanation of the observed progressive differences on the basis of thermal variation should prove valid*. Unquestionably this is true; and the proof afforded of close and actual stellar relationships serves to widen indefinitely the scope of conclusions regarding the nature and origin of sun-spot spectra.

They are not wholly made up by reinforcement. Weakened as well as strengthened lines are included in them, the same metals giving rise to both classes of effect simultaneously. The lines weakened, however, belong to a different category from the lines strengthened. They are almost exclusively of the "enhanced" sort; they are vivified in the spark as compared with the arc, and tend to become obliterated in an arc actuated by a feeble current. Their partial effacement in spot-spectra would then seem to satisfy the inference from the intensification of weak-arc lines; they imply, it is held, a diminution in spotted regions of thermal or electrical excitement. Yet this reading of the script encounters a difficulty. The weakened lines, since they in a measure *expunge* the corresponding Fraunhofer lines, cannot have an underlying locus of production. In other words, they necessarily originate above the reversing-layer. They must, as Mr. Fowler maintains †, emanate from the chromospheric gases. A great deal, it is true, has yet to be learned about them; and the remarkable effacement of silicon-absorption in spots ‡ is at present particularly unaccountable.

Thus, the argument for their lowered temperature rests entirely upon the character of the additional absorption distinguishing their spectra. It is strong, perhaps irresistible, notwithstanding certain anomalies adverted to by the Mount Wilson authorities. One is the behaviour of the blue line of calcium at λ 4227, which in the laboratory gains rapidly in relative importance as temperature declines, and in the red stars becomes a full match for H and K, yet retains its normal solar insignificance in sun-spots. Another (not mentioned in the paper before us) is the frequent emergence of helium-absorption in penumbral light. This can scarcely be accounted a sign of diminished temperature.

The distribution of heat among the various sections of the Sun's surroundings does not appear to follow any obvious rule. The facts so far ascertained, indeed, flatly contradict what might have been anticipated. Up to a certain point, the temperature-gradient seems actually to rise with increasing distance from the Sun. There is, for instance, some reason to believe the reversing layer to be cooler near the bottom than at the higher levels whence the eclipse "flash" is derived. And the spot-spectrum, which takes its rise from beneath the *lowest* of the reversing strata, corresponds, by a consensus of authorities, to a still further thermal drop. But if so—if spots are cooler than the reversing envelope above them—the Fraunhofer lines due to that envelope ought, one might suppose, to show over spots as a range of bright rays. They certainly do not do so as a whole; although Dr. Mitchell's observations of a select class of "reversals" may in part meet the requirements of the case. They affect chiefly broadened and faint lines. "The widened line," he tells us *, "is split in two through the spectrum of the spot, the central portion being bright. The fainter lines are usually the most strongly reversed. The widening of these lines is considerable, being sometimes ten times the normal width of the line. Many of the reversals have been seen not only over the umbra itself, but even more distinctly over the penumbra, and over the photosphere in the immediate vicinity."

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* *Astroph. Journ.* vol. xxiv. p. 77.

† 'Monthly Notices,' vol. lxvi. p. 361.

‡ Hale and Adams, *Astroph. Journal*, vol. xxiii. p. 30; Mitchell, *ibid.* vol. xxii. p. 30.

This last circumstance is very perplexing, since, by Kirchhoff's law, the photosphere must be hotter than either spot-cavities or the reversing layer, both of which show absorption-spectra projected against its prismatic background. Mitchell's reversals have not yet been recorded on Mount Wilson: but with an eight-inch grating soon to be installed there, further, and presumably successful, search for them will be made. Their discernment and explanation are plainly of fundamental importance in the arduous task of unravelling the intricacies of sun-spot spectra.

AGNES M. CLERKE.

The Sun-spots of 1906 *October.*

THE Sun during this month passed through a pronounced secondary minimum in sun-spot activity; indeed the only important disturbance was one which had commenced in September—viz. that formed by Groups 5979 and 5983, with Group 5986 which arose later to the west of them. These groups had all passed out of sight at the west limb by October 7, and from that date until quite the end of the month the Sun was almost entirely free from spots; and, indeed, was completely so from the 17th to the 23rd and again on the 27th and 28th. On the 29th a fine stream, Group 6001, came into view at the east limb, and a period of revival commenced.

Rotation 710 began October $22^{d}\cdot02$.

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* *Astroph. Journal,* vol. xxii. p. 8.

Spot-Groups in 1906 October.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	d	°	°	
5986	Oct. 2	Oct. 5	4	Sept. 29·7	294	−18	Short stream *p.* Gr. 5983.
7	4	7	3	Oct. 7·2	196	+ 9	Very small spots. Intermittent.
8	4	5	2	8·8	174	+16	A few very small spots.
9	7	10	4	7·8	187	−20	Short irregular stream.
5990	8	10	3	8·0	185	+14	Small spot.
1	8	8	1	8·4	180	+15	Very small spot.
2	10	10	1	4·3	234	−10	A few very small spots.
3	11	11	1	7·1	195	−12	Very small spot.
4	11	12	2	8·7	176	−17	Pair of small spots.
5	11	12	2	10·7	150	+14	Very small spot.
6	14	14	1	9·9	160	−19	Very small spot.
7	14	16	3	10·6	151	+26	Short stream.
8	15	16	2	12·2	129	−11	Very small spot.
9	24	26	3	22·1	359	+10	A few small spots.
6000	24	24	1	28·8	270	+10	Very small spot.
1	29	Nov. 10	13	Nov. 4·5	182	+14	Fine straight stream.

Notes on the Principal Spot-Groups.

Groups 5979 and 5983, which originated in September, increased in the early days of October, and together formed a long irregular stream, the principal member of which, with an area of about 120, corresponded to Group 5979 and was in the rear of the stream.

Group 6001.—A fine straight stream, 13° in length. A revival of Group 5990. Area about 150.

E. Walter Maunder.

Fireballs in January.

During the last few years, and particularly in and since 1900, the month of January has furnished a marked abundance of fireballs *. The epochs of apparition have been chiefly as under:—

January 9–10,
13–15,
25–29.

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* A previous letter on this subject by the present writer was published in the *Observatory*, xxviii. p. 51 (1905 January).

No specially rich shower appears to have been visible, but further investigation may reveal several above the average intensity. Radiants have been indicated in the second week of the month in Monoceros, Hydra, Cancer, and Ursa Major, while, near the close, the paths have been directed from various constellations, including Cepheus, Gemini, the Lynx, &c. But in some instances the exact centres of divergence have not been determinable owing to the nature of the observations, which have not always been sufficiently definite and accurate for purposes of computation. Spectators of these objects have, in fact, frequently been content to allude to their general aspect and position without giving really useful details as to the beginning and end points of the flights.

As a rule, the meteors have been slow-moving and long-pathed, with radiants not very far above the horizon and in the western half of the sky.

More observations are needed, and it is hoped that with suitable weather a few very brilliant meteors may be noticed in 1907 January, at the particular epochs indicated. It would be a great advantage if foreign observers cooperated in gathering materials. The evidence obtained in the last few years merely relates to the very small area over which the British Isles extend. It is only permissible for us to witness, from our very contracted view-point, a few drops of the widespread luminous shower affecting the Earth's surface as a whole. Though a good deal may be seen in English skies alone, it must be far less than a thousandth part of the aggregate of meteoric phenomena displayed over the entire globe.

Even two or three drops, however, sometimes suffice to determine the direction and character of a shower, and additional observations are therefore recommended as likely at least to afford clues both as to the radiants and dates of greatest abundance of January fireballs. And it is fortunate that for the detection of these striking bodies it is not significant whether or not the Moon is up or whether fog or thin cloud partially veils the sky. They can be well seen in spite of such hindrances. The chief necessity is that observers, whenever such objects are noticed, should record their positions, directions, and durations of flight with the utmost accuracy allowed by the prevailing conditions. The points of appearance and disappearance in R.A. and Dec., or relatively to stars near the apparent flights, should be registered immediately, for without full and reliable materials of this sort descriptions of meteors are practically valueless.

With a greater number of observers watching for bright meteors of the kind alluded to and suitably recording them, we should soon amass a plentiful harvest of such materials, and meteoric astronomy would receive substantial benefit.

Bishopston, Bristol,
1900, Dec. 4.

W. F. Denning.

The Quadruple Stellar System of κ Toucani *.

THE quadruple system of κ Toucani and Lacaille 353 is referred to in M. N. R. A. S. vol. lvii. pp. 456–7 (1897). Through the courtesy of Sir David Gill, I have recently obtained a measure with the Victoria Telescope of the smaller set of this system. The position-angles so far observed are:—

1896·2	182° ±	Lacaille 353.
1900·0	195 ·1	
1900·8	199 ·8	
1902·0	207 ·5	
1906·7	228 ·6	

These indicate an increase of some 5° p. a. The distance is about 1″.

The position-angle of the pair κ Toucani is decreasing at the rate of $\frac{1}{3}$° p. a. The distance of the components is about 5·5 that of the pair Lac. 353 (Innes 27).

To gain a first idea of the order of this system, we may assume that the motions in angle and the distances are the mean motions and mean distances. The factor of uncertainty may be large, 10 or so, but it is more probably about 2.

Let the sum of the masses of the two components of κ Toucani be M, and of Lac. 353 be m; the period of κ Toucani be 1000 years, and of Lac. 353 be 72 years; then

$$1000 = 5{\cdot}5^{\frac{3}{2}}/\sqrt{M} \quad \text{and} \quad 72 = 1/\sqrt{m},$$

hence

$$m = 1{\cdot}2\ M,$$

or the mass of the fainter pair is the greater. The two double stars are 320″ apart; and if we assume that this is about their mean distance, the period of revolution of the quadruple system will be about 300,000 years.

It will be noticed that the revolutions of the two pairs are in contrary sense.

R. T. A. INNES.

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* [As a double star κ Toucani consists of a 5·0 magnitude accompanied by a purple star of magnitude 7·7. Preceding this pair is another star, Lacaille 353, found by Mr. Innes in 1896 to be a close double. This pair have the same proper motion as κ Toucani, notwithstanding that they are 300″ apart.—T. L.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Periodical Comet due in 1907.

Gentlemen,—

Both the periodical comets expected in 1906 duly returned. Finlay's, which was discovered in September 1886 and observed at the return in 1893, but not during the next, when it was unfavourably placed; and Holmes's, which passed its perihelion in March, but did not manifest its presence (as M. Schulhof thought was probable) until more than two months afterwards when drawing towards opposition to the Sun, and even then required the assistance of the camera to make it perceptible. Prof. Max Wolf found its track first registered on a photographic plate taken on the 28th of May at the Königstuhl Observatory, Heidelberg.

Only one is due to return in 1907, and that not until nearly the end of the year. The body in question was discovered by M. Giacobini at Nice (who had then recently received the Lalande Prize for previous discoveries of comets) on the 20th of December, 1900. It was a very faint telescopic object and presented only the appearance of a small roundish nebula without tail. Prof. Kreutz investigated the orbit and found it to be elliptical with a period of about seven years. The comet had passed its perihelion on the 3rd of December, seventeen days before discovery, so that a return will be due in that month of 1907.

Yours faithfully,

Blackheath, 1906, Dec. 7. W. T. Lynn.

The Helium D_3 Line in Sun-spot Spectra.

Gentlemen,—

It is nearly a year since (see *Observatory*, No. 360) I expressed my conviction that, judging from many observations of the appearance of the helium D_3 line in absorption in the neighbourhood of active spots, this phenomenon should also be seen over areas *without* spots being near by. I stated in the same letter that I often see great distortions and serrations of the hydrogen Hα line over parts of the disc where no spots are; and speculating on the clue this affords for the likely presence of D_3 in absorption, I ventured to express the above-stated belief. A *résumé* of further observations made by myself, also mentioning those made by Prof. Fowler and by Prof. C. Michie-Smith, of Kodaikanal Observatory, and his assistants, you kindly allowed to appear in your issue for December 1905 (No. 364).

I may say that since that time I have paid particular attention in my spectroscopic observations to parts of the Sun's surface which at the time showed no spots, still, of course, keeping within the activity-zones. In fact I selected faculic areas (leaving out those in very high latitudes), and succeeded in seeing D_3 well on at least a dozen occasions, apart from a greater number of instances in which its appearance was not strong enough to make me feel absolutely sure. The cases where I saw D_3 specially well include about three, in which Hα was at the same time also greatly affected, although no spots, of even the smallest size, could be discerned at the time; but in the remaining cases this tell-tale was also absent. A growing conviction forces itself upon me that D_3 in absorption makes its appearance on rather small provocation, and that there is just a possibility of D_3 being represented after all constantly in the solar spectrum by a dark Fraunhofer line, only our instrumental means have so far failed to bring it out.

To begin with, there is no *prima facie* reason why helium should not conform to Kirchhoff's law of radiation and absorption. Secondly, we are assured (see Scheiner's 'Astronomical Spectroscopy,' p. 383) that the best photographs of the solar spectrum show D_3 represented by a faint *bright* line. If this is so, the supposition becomes extremely urgent, that this bright line is bisected by a dark absorption-line. Thirdly, we should remember that we have in the violet portion of the Sun's spectrum two abnormally strong lines, viz. the H and K calcium lines; and, arguing from the point of apparent extremes of strength, it is not unscientific to allow for extremely fine absorption-lines as the opposite extreme, such excessively fine lines having so far escaped our instrumental scrutiny.

I venture to submit that an instrument like the Michelson Echélon Grating Spectroscope carefully applied to region of D_3 in the solar spectrum would help to elucidate the existing uncertainty. Or could the interferometer be applied to the same end?

We know already that D_3 is a double line, having a fainter companion on the less refrangible side, the wave-length of the principal line being 5875·870 and that of its red companion 5876·209 (see Kayser's 'Handbuch der Spectroscopie,' vol. ii. p. 560). The difference is thus 0·339, which, halved and added to the lesser wave-length, gives for the centre of the combined lines a wave-length of 5876·0395, or extremely near to the value given by Young, which Scheiner tabulates in the revised list of the chromospheric lines at 5876·0.

I mention this, because I learn from Father Cortie that he has measured the wave-length of what appears on a photograph he has succeeded in taking of a spot-spectrum to be an absorptive effect of D_3, but he informs me it seems not in exact agreement with the position D_3 should occupy in the spectroscopic scale. I may explain at this juncture that not in a single instance have I seen what I firmly believe to be D_3 *in a spot-umbra*, but invariably in the area a considerable distance away, and, as stated above, also over areas in which no spots were visible at all anywhere near.

In this particular characteristic the observations taken at Kodaikanal Observatory bear me out, and in the list Prof. C. Michie-Smith gives of *spot*-spectra observed there is not a single instance where D_3 is mentioned as having been seen widened, in fact its visibility is not alluded to at all. Thus the photograph mentioned may possibly show another thing to what now a number of quite independently working observers have seen very frequently. That photograph was taken on July the 31st last, and on that particular day, and indeed on previous and following days, I saw D_3 in the neighbourhood of some passing active spots exceptionally well, *but not in the umbræ.* The appearance of D_3 on most occasions would also not imprint itself in the manner in which it has done seemingly on Father Cortie's plate. In by far most cases I see D_3 in the unconnected portions of a broken line, which makes its appearance synchronous with special outbursts, and is far from constant and enduring. No one who did not happen to be, by the mere luck of accident, altogether in readiness with his tele-spectroscopic camera for taking the picture could succeed in securing one at all. Still it is not altogether impossible; but I believe that what Father Cortie has succeeded in photographing is of a more "*quiescent*" nature. There are certainly in close proximity to D_3 a number of other lines, the origin of which is still in doubt, and one or the other of these may be affected in the way Father Cortie's photograph shows, which is a distinct widening effect, such as we often see across active spot-umbræ in the sodium lines.

So far I have not paid any attention to the behaviour of the other helium lines, but shall certainly do so, since it is very likely that when D_3 shows well they should also exhibit signs of being affected; and if this is found to be the case, this would argue in turn for the *bona fides* of having the true D_3 under observation.

Anyhow, I trust that, now increasing interest is aroused in this matter, further observations and discussions will help to dispose of what so far seems to constitute an extraordinary anomaly in the solar spectrum.

Yours respectfully,
ALBERT ALFRED BUSS.

2 Lansdowne Terrace,
Grosvenor Square,
Ashton-on-Mersey,
near Manchester.
1906, Nov. 14.

The Eclipse of August 31, 1030.

GENTLEMEN,—

Now that the date of the battle of Stiklestad is coming to the fore again through Mr. Cowell's eclipse investigations, perhaps I may be allowed to comment on Dr. Dreyer's letter in vol. xviii. p. 363 of the *Observatory*. He there rejects the statements of the sagas, which I had quoted in my letter on p. 336 of the same volume, that the eclipse occurred at the time of the battle in which St. Olaf was slain, because the historians quoted by Dr. Konrad Maurer do not mention the eclipse (which we know occurred on August 31st), but give July 29 (the fourth day before the calends of August) as the day of the battle. He makes great account too of the fact that that day was set apart as Olaf's day in the Norwegian calendar; but as his canonisation did not take place until more than one hundred and thirty years after his death, that argument is of little weight. Some modern writers have confused the year; thus Larousse gives A.D. 1030 as the date when Knut (commonly called in English books Canute) conquered Norway, and 'La Grande Encyclopédie' says that Olaf invaded it in 1029 and was killed on the 31st August. Now our own (the Anglo-Saxon) Chronicle is sufficiently clear that Knut's conquest of Norway took place in 1028, and the attempt of Olaf to recover it, which ended in his death at Stiklestad, in 1030.

The great Norwegian historian, P. A. Munch, in his 'Det norske Folks Historie' (which appeared at Christiania in 1852–55, a short time before Maurer's 'Die Bekehrung des norwegischen Stammes zum Christenthume,' published at Munich in 1855–56) accepts August 31st, 1030, as the day of the battle. There is a little circumstance mentioned by Maurer (vol. ii. p. 536), but not considered by him to outweigh the statements of the historians, which certainly makes it very probable that the later date was correct on grounds quite independent of the eclipse. It is that one of the accounts speaks of the difficulty of identifying the body of Olaf when it was buried in the night following the battle.

Now the latitude of Stiklestad is very nearly 64°, and even at the end of July there would practically be no night there, though there would be a few hours at the end of August. I am sorry I cannot read Norwegian, and I do not think any translation of his history has been published, but his acceptance, after study of the authorities, of August 31 as the date of the battle (given also in a detailed chronological table at the end of the volume) is of great weight, and the circumstance I have just mentioned is a remarkable confirmation of it.

Yours faithfully,

Blackheath, 1906, Dec. 18. W. T. LYNN.

OBSERVATORIES.

[Continued from vol. xxix. p. 468.]

POTSDAM (Astrophysikalisches Observatorium). *H. C. Vogel.*—The 80-cm. refractor was in use for spectroscopic work. The change in the spectrum of ζ Boötis, as shown in the spectrographs, has been published in *A. N.* 4067. Twenty-six plates of ε Aurigæ for variable motion in the line of sight were taken and 20 already reduced. Professors Wilsing and Scheiner have continued their researches on the temperature of the brighter stars. Prof. Lohse during the winter made observations of the physical features of Jupiter on 20 evenings, and later of Mars.

The Photometric Durchmusterung of the Northern Heavens, undertaken by Profs. Müller and Kempf, was concluded by the completion of the zone +60° to +90°, the last observation being made on 1905, Dec. 31. Printing commenced in the early spring. The catalogue will contain 14,199 stars arranged in order of right ascension. The observation of variables has been continued.

Prof. Lohse took a number of photo-heliograms for sun-spot statistics.

The manuscript of the four Astrographic Catalogue volumes was completed, but the printing has been somewhat delayed by the computation of the plate-constants. The fifth volume is in progress. Prof. Lohse measured 71 double stars with the 30-cm. refractor, giving special attention to 70 Ophiuchi. Time-service.

VIENNA (von Kuffnersche Steinwarte). *L. de Ball.* — The meridian work is chiefly the observation of fundamental stars for the Vatican zone of the Astrographic Chart. The photographic refractor was in use for variable stars.

ZURICH. *A. Wolfer.* — Spots, faculæ, and prominence observation. The relative numbers are :—

	Days of observation.	Relative numbers.
1905	273	62·7
1904	278	41·1

Time-service.

ROYAL OBSERVATORY, CAPE OF GOOD HOPE.—The Report of this Observatory for the year 1905, which is possibly the last or at any rate the penultimate of the series that will be signed by Sir David Gill, comes in a different form to its predecessors. This one appears as a parliamentary paper and can be bought from Messrs Wyman & Sons for twopence. The gist of much of the matter it contains was described by Sir David at a recent meeting of the R. A. S. and reported in last month's number. Besides the subjects there referred to, there are paragraphs on the arrangements pertaining to the splendid clock which is now a feature of the equipment of the Observatory. This clock is contained in a nearly air-tight enclosure automatically maintained at a temperature of 75° F., which is managed by means of a thermometer the mercury in which meets a platinum wire as soon as the temperature rises to 75° F. This establishes an electric circuit and by help of a delicate relay cuts off the current from the electric incandescent lamps that heat the enclosed chamber. When the temperature falls in consequence, the lamps are relighted and the temperature rises. It seems that the clock is now acting successfully.

The Astrographic Catalogue work is proceeding and at the date of the Report 907 plates had been measured containing about a quarter of a million different stars. The Victoria telescope was devoted to the photography of star-clusters, nebulæ, and photographic observations of the satellites of Uranus and Saturn, and a number of good photographic observations of Phœbe near elongation have been secured.

The report gives brief details of the work done in the various sections of the survey of Africa. It appears that the field-work of the survey of the Transvaal and Orange River Colony is complete and the reductions nearly finished. Of the measurement of the arc on the 30th meridian, all the stations north of the Zambesi up to latitude 13° 58′ S. have been occupied. The results of the geodetic survey of Southern Rhodesia, from Bulawayo and Gwelo north to the Zambesi, have been passed through the press, and the Report of H.M. Astronomer on the Anglo-German boundary between British Bechuanaland and German South-west Africa has been sent to Berlin.

The difference of longitude between the Observatory and St. Helena, determined telegraphically by Messrs. Pett & Cox, is published in the report, and also the longitude of Accra determined by Major Watherston with the help of Mr. Pett.

PUBLICATIONS.

THE ASTROGRAPHIC CATALOGUE: OXFORD SECTION.—Another contribution to the series of volumes making up the photographic catalogue of the heavens has arrived, as will be seen from the account of the proceedings at the last meeting of the Royal Astronomical Society, when it occupied a full share of attention. As this note is intended to show specific points where the Oxford work differs from that at other observatories, it may be convenient to refer to previous notices which have appeared in these pages of other volumes of the series *.

All the co-operating observatories use *réseaux* imprinted on the plates. At Oxford this printing was done *after* exposure to the sky, and by means of a gas-jet or electric light at a distance of 15 feet, the *réseau* and plate being separated by an edging of paper 0·1 mm. thick. The light did not therefore consist of parallel rays, but the effect seems insensible. Usually the *réseau* lines are as nearly as possible 5 mm. apart, so that 1 mm. is equivalent to 1′ if the telescope is of a particular length, which is to some extent a matter of chance; but as it happened that the focal length of the Oxford telescope did not give the precise equivalent, Prof. Turner adopted the ingenious plan of having his lines cut 5·04 mm.

* Vol. xxvii. pp. 137 & 284.

apart. It seems that in figuring the *réseau* the central lines are numbered 13.

The essential difference in the practice at different observatories is the instrument of measurement. At Oxford, as at Greenwich, the distances of the stars from the *réseau* lines are measured by means of a scale; at all the other observatories which are measuring, so far as we know, a micrometer-screw is used. This is a method which some persons might quarrel with, as involving a loss of accuracy, but the use of the scale is justified by results. When the work of the astrographic chart was initiated, the Committee imposed the condition that the probable error of a measure (not of a deduced star-place) must be less than ±0″·20, and the probable error of the mean of two measures (and all the plates at Oxford are measured twice, first in a direct position and then reversed) is ±0″·14.

At Oxford, although, as at other observatories, the plates are exposed to the sky three times, once for 6 minutes, then for 3 minutes, and finally for 20 seconds, only the 6-min. image is measured, and the measure is given only to the third decimal place—the unit being a *réseau* interval or 300″. This is evidently what Prof. Turner referred to when he spoke of being on the "extreme wing of the party." At Greenwich, which is possibly the penultimate occupant of this wing, two images are measured, and the means, weighted by a method which it is not necessary to explain here, are taken to the fourth place. The reason for limiting the work at Oxford is found in the limited staff of the Observatory, and it is explained in the Introduction that as the error of measurement is only about a third of the error due to other causes it would have been disproportionate to have attempted more in this direction. The final test of the catalogue is the deduced place in R.A. and declination; for though the co-ordinates as yet printed are referred to rectangular axes, the individual plates are held together by stars whose positions are known in equatorial co-ordinates, and in this respect the Oxford catalogue depends for the moment on stars whose places were observed with the meridian instrument of Cambridge Observatory or those of the Leiden, and an illustration is given in the Introduction of the accuracy of the reduced measures. A certain star was shown on four different plates, and these plates were reduced by the usual formula. The resulting places of the star differed from the mean generally by one unit of measurement, *i. e.* 0″·3, in one case by three units. There are some larger discordances than these, for in an investigation which includes 27 stars there are a few discordances from the mean of four or five units. The result of this investigation gives the probable error of a deduced equatorial co-ordinate as 0″·39; but it is to be remembered that this includes the effect of imperfectly known places of the reference stars, and, as was said at the meeting of the R. A. S., the necessity for reobservation of these is imperative.

This comparison of places deduced from different plates calls attention to the fact that the Oxford Catalogue differs in one respect from Greenwich, and follows here the practice of our Continental colleagues. The plates were measured from *réseau* line 0 to 26, and not only as far as the lines where each plate meets its next neighbour in the zone, so that there is some overlap, and some stars appear twice in this volume with two different numbers. It should be said that this volume contains measures of plates only in the zone whose centres have declination +31°, so that the corresponding measures of the stars on the lower halves of the plates (namely, those of the images on the north halves of plates of zone +30°) will be in the next volume. The reference numbers do not begin with unity on each plate, but the stars are numbered consecutively through the zone, with the exception that the first star on each plate has the initial number of a century, so that there may be a hiatus of 99 between the last number of one plate and the first of the next. In fact, the last number of the last plate—there are 160 in all—is 73,884, although the titlepage says that the number of star-images is 65,750. The plate-constants, which are to be taken only as provisional, are given in the heading of each plate; and a feature of this catalogue which does not occur in others is that the standard co-ordinates of the reference stars, or, in other words, the places of the Cambridge or Leiden catalogue converted into the rectangular co-ordinate system, are given in a section at the end of the volume, the reference stars in the body of the work being indicated by an asterisk, although their reference-number is, of course, given in the subsidiary table, an undoubted convenience to anyone wishing to exercise himself in re-computing the constants. That the measures of 65,000 stars should have been packed into 223 pages argues for the compactness of the arrangement, and the work has not suffered in clearness. Anyone reading Prof. Turner's Introduction will understand the reasons for economy, so that nothing in the way of references to other catalogues has been given except in the addendum of standard co-ordinates as stated above. The whole work seems to call for congratulations to the Savilian Professor on having done so much with such small means, and to the staff because the end of a very arduous task is well in sight.

NOTES.

COMET NOTES.—Comet Holmes was photographed with the new reflector at Heidelberg on Dec. 7^d 7^h $8^m{\cdot}1$ local M.T., in R.A. 3^h 38^m 42^s, N. Dec. 51° 17′ 18″.

M. Schulhof continues his ephemeris of Finlay's Comet in *Ast. Nach.* 4140. The brightness is steadily diminishing, and in the middle of March is equal to that at the last observation in 1887.

1907.	R.A. h m s	N. Dec. ° ′	1907.	R.A. h m s	N. Dec. ° ′
Jan. 5....	7 22 17	27 24	Feb. 14....	6 46 32	27 26
13....	7 9 39	27 42	22....	6 47 16	27 12
21....	6 59 34	27 48	Mar. 2....	6 49 59	26 56
29....	6 52 24	27 46	10....	6 54 19	26 39
Feb. 6....	6 48 8	27 38	18....	6 59 59	26 21

The following are the latest elements of Comet 1906 *g* Thiele by Dr. E. Strömgren from observations on Nov. 10, 17, 25:—

T .... 1906 Nov. 21·26 Berlin M.T.
ω .... 8° 38′
☊ .... 84 56 } 1906·0.
i 56 32
log *q* ...... 0·0845

	R.A. h m s	N. Dec. ° ′
Jan. 1	15 14 52	59 32
5	15 38 0	60 0

Comet 1906 *h* (Metcalf) proves to belong to the Jovian comet family, the following elements and ephemeris (for Berlin midnight) being by M. Ebell (*Ast. Nach.* 4141):—

T .... 1906 Oct. 10·79 Berlin M.T.
ω .... 200° 41′·3
☊ .... 194 19·4 } 1906·0.
i .... 14 31·5
log *q* ...... 0·21231
e .... 0·578
Period .... 7·588 years.

	R.A. h m s	Dec. ° ′		R.A. h m s	Dec. ° ′
Jan. 4....	4 5 4	3 40 S.	Jan. 20..	4 16 48	1 9 S.
8....	4 7 26	3 6	24..	4 20 37	0 28 S.
12....	4 10 11	2 29	28..	4 24 43	0 14 N.
16....	4 13 19	1 50 S.	Feb. 1..	4 29 4	0 57 N.

The elements resemble those of other Jovian comets, but identity is not possible.

A. C. D. C.

Minor Planet Notes.—The following new planets have been discovered at Heidelberg and Taunton, Mass., U.S.A.:—

	Date.	Discoverer.	Magnitude.
WM	1906 Nov. 10	Kopff.	13·8
WN	14	Lohnert.	12·3
WO	12	Metcalf.	12·5
WP	13	”	11·5
WQ	24	Lohnert.	12·0
WR	14	Metcalf.	13
WS	14	”	13
WT	19	”	13

WQ may be identical with 167 Urda or 495 Eulalia.

A. C. D. C.

THE number of *Engineering* for December 21 contains a full description, with pictures and diagrams, of the equatorial twin telescope of the Radcliffe Observatory.

THE next Meeting of the Royal Astronomical Society will be on Friday, January 11; of the British Astronomical Association on Wednesday, January 30.

FROM AN OXFORD NOTE-BOOK.

DURING the summer of 1904 the writer was able to spend eight weeks at the Lowe Observatory in Southern California. A considerable portion of his time was devoted to a study of Eratosthenes. The interior was found to be seamed by numerous fine cracks. Watching some of these cracks soon after the Sun rose upon them, he was able to see them broaden out and change gradually into canals. It is his belief that the cracks gave out water vapour, which fertilized the vegetation along their sides and in their vicinity, and that it was the growth of this vegetation that produced the appearance of a canal.

The above paragraph is taken from p. 177 of a Memoir by Prof. W. H. Pickering to the American Academy on February 9 last entitled "Lunar and Hawaiian Physical Features compared"; the opening paragraphs of which explain that the summer of 1905 was spent in an expedition to the Hawaiian Islands in order to study their volcanic features, which he believed to bear a striking resemblance, in some respects, to lunar features of the kind. Thus, in the course of two consecutive summer expeditions, one devoted to a scrutiny of the Moon, and the next to an equally patient scrutiny of a portion of the Earth most nearly resembling it, Prof. W. H. Pickering has brought together a large and valuable series of facts bearing on the relationship between the two surfaces. Some of his conclusions will not, of course, be accepted at once: the paragraph just quoted, for instance, will meet with much scepticism and may even encounter ridicule. But in view of recent history, the writer of the paragraph can well afford to wait. It is not yet three years since the time when he was practically alone in claiming the existence of Phœbe: even less since his views on the manner of its birth were treated as a wild speculation; but in the sequel he turned out to be on the right side in both cases. Both his accuracy in observation and his sagacity in interpreting the results have thus been strikingly vindicated, and to these qualities we must certainly add a notable spirit of enterprise.

ACCORDING to a paragraph in the morning papers, the total wealth of the United Kingdom is £8,950,000,000, neglecting a few Government properties as an offset against the National Debt. The figures are conveniently near to 10^{10}, a magnitude for which it is sometimes desirable to have an illustration. A more astronomical example is the number of seconds elapsed since the invention of the telescope to the present time; and it brings to mind that we are approaching the tercentenary of that epoch-making event. Are our friends in Holland going to do anything about it? But as regards 10^{10}, in another aspect it represents the mistake made by confusing 9 with $\bar{1}$ or 8 with $\bar{2}$ in working with logarithms—a mistake so large that there seems little likelihood of committing it undetected. Consequently most astronomical computers have for many years used the symbols 9, 8, 7, &c. throughout in preference to $\bar{1}$, $\bar{2}$, $\bar{3}$, &c. There seems to be a modern tendency, however (as instanced in a recent edition of four-figure logs at Cambridge), to resume the negative notation, and it may be that it is found more comfortable to work with on trial. One thing is pretty clear: in work involving the extraction of (say) fourth powers, the possible mistake is reduced to moderate dimensions when it might pass undetected; so that the rigidly accurate notation may be preferable. But there are not many instances of such work in astronomical computations, and probably there is some better reason than this for the recent change.

ONE characteristic of the American newspaper reporter is that he does not shrink from telling the plain unvarnished truth, and we are thus enabled to profit by his researches. Writing of an astronomer in a paper of date November 15 (the name does not appear on the extract kindly sent me), the interviewer says of his victim that "—— has passed the fortieth milestone of life's journey, but, *unlike many persons who devote themselves to scientific ends*, is affable and charming of personality."

The italics are mine: it seems desirable to draw general attention to so important a characteristic of the scientific public as is implied therein. Of course each one of us can, as an individual, flatter himself that he belongs to the select few rather than to the "many persons." But escape is not so easy for groups and societies of "those who devote themselves to scientific ends," such as the Royal Astronomical Society for instance; and lest this point should be overlooked it is remorselessly made definite in another paragraph:—

> Members of that body [the R.A.S.], as a rule, are cold-blooded. They do not admit anyone to equality with themselves unless the right to comradeship has been clearly established."

So that we have at last been found out! And, as is sometimes the case with discoveries, more than one person has arrived at the truth about the same time. For this cold-bloodedness has been detected independently in quite a different corner of the world by another investigator, who, though he did not announce his conclusions so publicly, has given a circumstantial account of the method of research which I have been permitted to see. He endeavoured to join the Society, and approached one of the existing Fellows with a request to be proposed for election. The sequel is best told in his own words:—

> I had been an astronomical contributor to a journal here for some years and

> so I had some published articles on hand which I forwarded to him. He read them over and informed me that "the evidence presented, although undoubtedly of some value, was much too scrappy to justify him taking the initiative." That was the whole length of his letter.
>
> Then I volunteered to write longer articles, so as to enable him to judge decisively on the point of my eligibility or otherwise.
>
> He gave me no answer.
>
> Then I went to ——, where he resided, and made bold to introduce myself to him for the purpose of ascertaining if possible the reason of his reticence. I wanted to confront him, so that he could examine me, and I could get some idea as to whether or not his own astronomical knowledge was mightily superior to my own. Well! he didn't seem a bit glad to see me, and when I mentioned who I was he seemed in an awful flurry. And for the life of him he did not know how to shake me off. I asked him if he was busy. "Yes," he replied, "rather busy." "If that be the case," I said, "I do not want to encroach upon your valuable time." He told me to call again in a day or two, which I readily agreed to do and then left him in peace. Faithful to my promise I called again. I knocked at the door. There was no response. I knocked and knocked till my arm ached;—still no response, not the faintest apology of a sound greeted me. I went round to the back and renewed operations with two-fold vigour—but with no success. I had to give up. He got the better of me, for I have neither seen nor heard of him directly from that time to the day that now is.

This personal experience is too true to life not to carry conviction to any reader who is not prejudiced. The reporter's dictum we might have denied; but one cannot claim warm-blood for a man who would keep silence when the offer was made "to write longer articles," or when a fellow creature "knocked and knocked till his arm ached" and even "went round to the back and renewed operations."

BEFORE leaving reporters, it should perhaps be admitted that they sometimes get out of their depth: not, of course, in gauging human character—there they are thoroughly at home; but in dealing with scientific matters of a technical kind they sometimes do not succeed in conveying to their readers the exact impression intended by their informants. I trust this is not putting the case too strongly: perhaps an instance will express more exactly what is intended. The following extract is taken from an article entitled "How I weigh the Sun," by the Director of the Echo Mountain Observatory (*Tit-Bits*, August 18), but only a few paragraphs are in the actual words of the Director: the others represent the interviewer's impressions of what he said; and here are a few of them:—

> [Seven sextillions], of course, is a tremendous number of tons for any moving mass to weigh, but there is a time twice each year when the Earth actually weighs nothing at all. In October last this Earth gradually began to lose weight like some huge giant dying of a decline, until at a certain moment of time it weighed only an ounce, then half an ounce, then a quarter, and finally, just fo about the fifty-thousandth part of a second, it weighed absolutely nothing whatever—not even so much as a soap bubble which a baby might blow away.
>
> To realize this you must remember that the Earth does not travel round the Sun in an exact circle, so that the distance from the Sun is always varying,

which, of course, alters the power of attraction or, in other words, the Earth's weight. But in October and April of each year the Earth is at an exact average distance during the fraction of a second, at which time, as I have said before, it weighs nothing. How short a space of time this is may be judged from the fact that the Earth moves at the rate of eighteen and a half miles every second.

But to weigh the Earth in the manner suggested would be a very costly matter, and so it is found to be more satisfactory to employ mathematics, when we shall arrive at the same result, assuming, of course, that we are correct in our deductions.

THE *New Album* for September 1906 contains an interesting account of an astronomical pilgrimage paid by the Bucks Archæological Society to Observatory House at Slough. Prof. Alexander Herschel showed them the remaining portions of the famous 40-ft. telescope constructed by William Herschel in 1786–7, and gave them an interesting account of the life and work of his grandfather.

In the same magazine for January 1906 there had already appeared an appreciative article on Caroline Herschel, from which I extract a paragraph in order to ask a question. This is the paragraph:—

> We hear of his grinding for sixteen hours at a stretch at one of his telescope-mirrors, and of Miss Herschel reading to him as he laboured. The works selected were generally the "Arabian Nights," or the novels of Sterne and Fielding. Reading would be interrupted for Miss Herschel to put bits of food into her brother's mouth, while he continued his monotonous labour without removing his hands. He had conceived the idea of a new form of telescope, and was labouring with almost frenzied energy to put it into execution. He succeeded so well as almost to revolutionise modern stellar astronomy.

The story is a familiar one, but I have several times asked, and so far unsuccessfully, what was the reason for these long spells of continuous grinding? It was not Herschel's usual method of work. We have at Oxford Vol. I. of his original records of the grinding operations, presented to the University Observatory in Prof. Pritchard's time by the present Sir William Herschel; and on turning over the pages one usually finds records of grinding for a few minutes only, and of the mirror being then left to cool for testing. Was the sixteen hours' continuous grinding a special investigation? Can anyone give a reference throwing light upon it?

FROM the *Bristol Evening News* for November 20:—

CRICKET SIDE OUT FOR NO RUN.

> In a recent cricket match at Capetown the Observatory eleven were all dismissed without scoring a run, Abrahams, the Rondesbosch bowler, taking seven wickets for 0. What makes it more curious is that in their first innings the Observatory mustered 64, to which Rondesbosch replied with ten more. Thus an astonishing victory by an innings and ten runs resulted. This was the first time a South African team had failed to score, though a De Aar side were once dismissed for 1.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. FEBRUARY, 1907. No. 380.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1907 January 11.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder. Fifty-one presents have been received since the last Meeting. The list includes a book by Prof. W. H. Pickering entitled 'Lunar and Hawaiian Physical Features,' presented by the author; a further series of enlarged reproductions of the Astrographic Chart, presented by the Royal Observatory, Greenwich; and an engraving from a portrait of Sir William Huggins, presented by the Subscribers to the Huggins Portrait Fund.

Mr. Maunder presented the usual annual report of the work of the Photographic Department of the Royal Observatory, Greenwich. He said:—The present report, which is for the year 1905, contains several features of more than ordinary interest. After the pronounced minimum of 1901 the rise in the solar activity proceeded but slowly until the sudden appearance of a gigantic group in October 1903. After that there was a steady, but by no means rapid, development of activity, unmarked by any great outbursts, throughout the year 1904. But with 1905 a new phase of activity set in, and the chief feature of that year was the great number of spot-groups of abnormal extent which were observed. Of groups large enough to be easily detected by ordinary sight, without optical assistance, no fewer than thirteen, visible in all on more than 80 days, were recorded. The largest group ever photographed at Greenwich ran its course from 1905 January 29 to February 11; the group of 1905 October 14–26 was practically equal to the great group of 1892 February, hitherto much the largest on the Greenwich register.

Mr. Maunder then showed on the screen photographs of the principal spot-groups of the year. He then proceeded:—Towards the end of 1905 the type of activity underwent a change. In November 1905 the chief feature was no longer the presence of one or more giant groups, but the tendency to form long processions of groups of moderate size, following each other at short intervals along a parallel of latitude, and this tendency has continued to be in evidence throughout 1906.

The question arises as to whether 1905 is to be regarded as the year of maximum or not. In the two previous cycles the northern hemisphere developed a great increase of area before the southern, and then showed a slight decline. During this slight decline of the northern hemisphere, the activity of the southern became very great, the spotted area south of the equator being about twice that of that north of it, and this great southern activity marked the time of the general solar maximum. In 1905, though there was an increase in both hemispheres, it was chiefly in the north, and if the precedent of the two earlier cycles was followed in this particular, the maximum is still to come—probably in 1907 or 1908. On the other hand, the mean daily spotted area of 1906 was only about three-fourths of that for 1905, a very marked decline, whilst the mean distance from the equator of all spots had already come down to 13°—as low as, or lower than, is usual at maximum. On the whole, therefore, it appears most probable that the maximum of this cycle will eventually turn out to have taken place in October 1905.

Mr. Bryant. I should like to say that though the maximum of magnetic storms may seem to have been in 1903, since when there has been no very great movement, still I think it not impossible that the curve showing the daily range of the movement of the magnets will follow the sun-spot curve closely, as at previous maxima. The observations are not finally worked up yet, but I think this will be the case. If this is so, it may be taken as confirmation of the "Leyden jar" theory.

Dr. Spitta. Will Mr. Maunder tell us something of the details of the photographs—the kind of plates used, the developer, and the length of exposure given?

Mr. Maunder. The photoheliograph in general use at the Royal Observatory is the one presented by the late Sir Henry Thompson, of 9 inches aperture and 9 feet focus, and used with a positive enlarger so as to give an image of the Sun 7½ inches in diameter. Occasionally a Dallmeyer photograph of 4 inches aperture is used instead. This instrument has a focal length of 5 feet, but the image of the Sun is practically of the same size as with the Thompson. Both instruments are usually stopped down—the Thompson to 4 inches, the Dallmeyer to 3 inches aperture. The plates used are the slowest lantern-plates obtainable in the market and are developed with hydroquinone. The great difficulty in solar photography is getting the exposure sufficiently short, and in summer-time the normal exposure given is equivalent to the $\frac{1}{3000}$ part of a second. This is in order to bring up spot detail. To bring up granulation detail, as in M. Janssen's photographs, an exposure shorter still must be given. Occasionally a few photographs of parts of the Sun's surface have been taken with the Thompson 26-inch photographic refractor. The aperture has, in these cases, been stopped down to 12 inches or less, and a negative enlarger employed, giving an image on a scale of nearly 30 inches to the solar diameter. Some of these photographs have borne enlargement afterwards to a scale of 7 or 8 feet to the solar diameter.

The President. In thanking the Astronomer Royal for sending this paper, and to Mr. Maunder for his description, I should like to take this opportunity to say what a very great advance appears to have been made by the use of slow plates for solar photography. At the time when more rapid plates were used there was little of the beautiful detail of spots and faculæ, such as we saw in the photographs thrown on the screen just now.

The Astronomer Royal. I present a joint paper in which Mr. Eddington and Mr. Davidson have worked with me in carrying through a very difficult discussion which was forced on us in connection with the Eros photographs, so that I do not wish to claim the credit of this paper for myself. The investigation here described was undertaken in connection with our Greenwich photographs of Eros 1900–1901. The determination of division-errors of the réseau as imprinted on the photograph plates to the high degree of accuracy required is in some respects a new problem, so that some of the methods may be interesting, and this is our excuse for inflicting a rather heavy paper on the Society.

The errors of the réseau itself were originally determined by a method which is described in the 'Astrographic Catalogue,' vol. i., and these were used as corrections in the reduction of the observations to deduce the places of reference and comparison stars, and for that purpose they were of sufficient accuracy. But it was felt that, in order to get a trustworthy value of the parallax, it was essential that the errors of the particular rêseau-lines between which Eros itself falls should be determined with the highest possible accuracy. That was necessarily left to stand over till the measures had been completed, and the work has now just been finished as far as we can carry it. I think, generally speaking, it has been considered sufficient for ordinary purposes to consider the division-errors of the réseau itself, and to take it that these will be transferred to the photographic plate; but as the highest degree of accuracy was required for the Eros photographs, a preliminary comparison was made in the beginning of 1903 of the réseau as photographed on seven plates with the réseau itself, and that indicated that there were serious differences which called for further investigation. Perhaps I might here explain how the réseau is printed on the photographic plate, and how the plates are taken, because on that depends the question of the effect of division-errors. I would now mention that under ordinary conditions the plate has one ground edge, which is placed in contact with the bearing-studs in the camera and also in printing the réseau. When the telescope is reversed the réseau on the plate is reversed relatively to the sky, so that unfortunately it happens that when we compare photographs taken east and west of the meridian with the telescope east and west of the pier, the division-error of the central line or lines comes in with double effect, and that, therefore, this division-error enters directly into the determination of the parallax; and if we want to get the parallax to one-hundredth of a second, we require to determine the division-error of this particular line to something of the same degree of accuracy; well, that was a very difficult problem. There were three steps in the determination. First, a more accurate measurement of the errors of lines 13, 14, 15 on the silver réseau; those are lines with which we are particularly concerned, for Eros was placed between 13 and 14 or 14 and 15, nearly in the centre of the plate. Secondly, the measurement of the systematic differences between prints of the réseau and the réseau itself. And thirdly, an examination of the curvature of the réseau-lines both on the réseau itself and on the prints. The numerical values of the errors given in this paper are expressed in seconds of arc for the scale of the Astrographic telescope, in which one millimetre is a minute of arc, and they must be halved before being applied to photographs taken with the Thompson telescope. The réseaux are all printed by using the object-glass of the Astrographic telescope to make the rays parallel, but the errors deduced here are applicable to the Thompson photographs equally well, only they have to be halved. I would explain further that the aim of this discussion was not to determine the errors of individual lines, but to refer the lines 13, 14, and 15, with which we are specially concerned for Eros, to the means of lines used respectively for comparison-stars and reference-stars. For comparison-stars the field was limited practically to lines 9 to 19, 14 being the middle line; for reference-stars it was more necessarily extended; it was limited to 55′ from the centre and practically it extended from lines 4 to 24, so that the aim has been to determine with the greatest accuracy the correction of lines 13, 14, 15 referred to the mean of 9 to 19 and 4 to 24 respectively. The individual errors of these lines had been determined with a considerable degree of accuracy, and it may be taken, we concluded, that the residual errors which are left over may be treated as accidental errors which will go out in the mean, so that we have these central lines referred to the mean of 20 lines, and we trust the accidental residual errors will go out in taking the mean of 20. For the new determination the interval between line 14 and each pair of lines from 4 to 24 was compared by means of the glass scale, so that line 14 was referred in succession to the pairs of lines 4, 24;

5, 23;......13, 15. Thus ten independent determinations were made of the residual division-error of line 14 referred to a pair of lines.

[Tables of the errors were shown on the screen, and the Astronomer Royal proceeded:—] Similarly line 13 was referred to each pair of lines from 3 to 23, and line 15 to each pair from 5 to 25. Since by this method each line is referred to pairs of lines symmetrically situated about itself, lines 13 and 15 could not be referred to the same lines exactly (4 to 24) as line 14, but the accidental residual errors arising from the substitution of line 3 for line 24, and line 25 for line 4, in the two cases respectively, is negligible in the means of 20 lines.

In this Table we have given three figures going to the thousandth of a second, and it says a great deal for the accuracy of Mr. Davidson's measures—for he made these measures—that we are able to deal with the thousandth part of a second in these results. I may here explain that we are getting to very small quantities in these determinations. The hundredth part of a second of arc is about one-third of the wave-length of ordinary light, so that when we get to the thousandth of a second it is a very small fraction of a wave-length. I do not profess that we can answer for the thousandth of a second, but it is well to exhibit it here.

Sir David Gill. Approximately determined division-errors were primarily applied?

The Astronomer Royal. Yes. As a matter of fact, the residual corrections are relatively small, but for the purpose of deducing an accurate value of the parallax they are important. They are to be considered as further corrections to be applied for division-error according to the position of Eros on the plate.

The next stage was to compare systematic differences between the silver réseau and its photographs, and for that 15 plates were selected so as to be well represenfive of the Eros plates taken throughout the Opposition. These were compared one by one with the réseau, the corresponding lines on the plate and the réseau being compared at y 13·5 and y 14·5. I should have made the point clear that Eros always falls between lines 13 and 15 in both co-ordinates, so that for this purpose it is sufficient to make the comparison at y 13·5 and 14·5. It will be seen these give relatively large corrections to the measures of Eros, and they will affect the value of the parallax. I should mention here that the great advantage we seem to have gained from the use of the Thompson refractor is that the scale is double, so that the errors of the réseau are halved.

The next question was the question of the curvature of the réseau-lines, because, as Mr. Eddington pointed out, since Eros is in the centre of the field, if it is referred to stars which are spread over the area y 9 to 19 any curvature of the lines will affect the result. The first step in this was to determine the curvature of the actual réseau-line 14 by means of a spider-line in somewhat the same way as Sir David Gill had done at the Cape, only in this case we were able, by mounting the spider-line alongside and parallel to the plate, to compare them directly. In order to eliminate instrumental errors of the micrometer, the réseau was reversed by turning through 180° in its own plane, and the spider-line was also reversed, but about its axis, and these are the results obtained.

Four determinations agree in giving a minus correction, but the quantity is very small. I should first have explained that the line was compared at one end taking y co-ordinate 4, 4½, 5, 5½, 6; and then again at 12, 13, 14, 15, 16 for the central part; and then again at the other end; so that there were five measures at each end.

The next step was to do the same thing with the prints, and for that purpose a scheme was drawn up for measures of eight photographs so as to be representative generally of the réseau lines and of the Eros photographs. Our object was to get rid of systematic errors which would affect the whole series of photographs, trusting that accidental errors would go out in the mean of the whole number.

The main result of these measures, taking the mean for the area 9 to 19, which is that covered by comparison-stars, is an apparent correction of 0″·006 with a probable error of 0″·005, which, I think, will be considered a satisfactory result to arrive at.

There is another investigation which Mr. Eddington undertook; but it is rather difficult to explain. In this he discussed the residuals of comparison-stars from photographs selected on days when there were morning and evening observations, at least two photographs east and two photographs west, and deduced from them what we may call the spurious parallax. I am only able to give a specimen here of a rather large table, but each of these numbers represents the sum of the division-errors in the two positions when the réseau is reversed.

The mean residual is 0″·05, and that represents practically the mean error of measurement of a single photograph, which, I think, is very satisfactory, and these results from the residuals of the comparison-stars are in satisfactory accordance with those obtained by direct measurements.

Sir David Gill. I think the Astronomer Royal and his Assistants who are associated with him in this paper ought to be congratulated upon the thorough way in which they have tackled this fundamental question connected with the discussion of the observations of Eros. There is no doubt about it, when you come to consider these plates and the réseaux printed on them you have to deal with a particularly small quantity. The problem before us is really very similar to that which arises in determining the intervals between the wires of a transit instrument. The thing to be sought for is not the precise position of the individual line, but what is the precise distance between the centre line and the mean line. That is a very sound principle to go upon. But the crucial test after all is what we have got in this last diagram, which, as the Astronomer Royal explained, shows the corrections derived from observations of the stars, and these correspond with corrections derived from investigation of the errors of the réseau-lines. We must not forget we are dealing with the question of parallax, and the Astronomer Royal's result will be diminished in its final effect, because these differences have to be divided by a factor which is generally larger than unity.

Prof. Turner. I think, Sir, I should like to repeat a remark I made last time: we who are doing the Astrographic Catalogue work find ourselves very fortunate in having this other investigation going on. The errors which the Astronomer Royal has brought to light, although serious enough in work on parallax of Eros, are, as a matter of fact, small for the purpose of the Catalogue. It would have cost us a great deal of labour to determine them, and similar errors have been neglected throughout our work. It is therefore comforting to have a thorough investigation made which is not only serving a useful purpose in itself, but reassures us by showing how small these errors are. I was looking up some references this morning, and I think the errors which I saw in the Astronomer Royal's paper are small compared with those which have been found in some similar cases.

Mr. Hinks. I have been much interested in the paper of the Astronomer Royal, because Mr. Russell examined our réseau at Cambridge some little while ago by a slightly different process, which has some merits. We cut a strip out of one of our réseau copies and made this the standard scale, carefully determining the errors of division in the usual way. We then superposed this successively on a number of our plates, determined the error of the copy on each plate by direct comparison with the standard scale, and hoped, by taking the mean of a large number of copies, to eliminate the accidental gelatine distortions and arrive at a knowledge of the systematic errors of the photographed réseau, as distinct from those of the original. We came to the conclusion that the method worked for the centre of the plate, that the gelatine distortions averaged out and left the errors of the projected réseau outstanding; but within about an inch of the edge the accuracy of the copies goes all to pieces, just as they seem to have found at Greenwich. But we were disposed to attribute the falling off, not to a decrease in the accuracy of the projected réseau, but to a rapid increase in the gelatine distortion, and to its becoming semi-systematic near the edge, as might indeed have been expected. In the central parts of the plate our projected réseau seems to be, if anything, even a little better than that of the Greenwich réseau; the accuracy of these Gautier réseau is really extraordinary.

The President. It might be well to have plates much larger than the réseau itself, so as to use only the middle of the film.

Sir David Gill. It is not a very serious matter if it is really a question of the contraction of the film, because we have to deal with the distance between two lines which include a star, and if any change takes place in the film itself, the star-image is displaced along with the lines. Thus if the distortion inside a particular square of the réseau can be considered uniform, no error in the resulting position of the star would result from a contraction of the film.

Prof. Turner. I should like to emphasize the point that really is the most important thing we want to know: Does the contraction take place during development? I think myself it does; but I do not think we have *proved* it. If the contraction takes place after the stars and the réseau have been photographed they go together, and, as Sir David Gill says, it does not matter. Similarly it does not matter if you take a paper print with the réseau on it: paper expands enormously in one direction as compared with the other: but you can get good star-places from the print, since stars and réseau-lines go together. But I think it is important we should be sure on that point. If the film alters in any way between exposure to the réseau and that to the stars, then it is a very different matter. I am sorry to say I did not hear the whole of the Astronomer Royal's paper; perhaps he may have said something to throw light on that point. Anyhow I think it is the next most important thing to be investigated.

Sir David Gill. If I may ask a question, can the Astronomer Royal account for the large difference of scale-value between the copy and the original correction?

The Astronomer Royal. I think it quite possible that the development of the gelatine film may cause it to expand or contract and cause different scale-value, but at the same time what strikes me, I do not know Mr. Eddington's views about it, is that this is a progressive change which proceeds continuously from the centre outwards; there is certainly an apparent jump at this extreme line, but it is a badly cut line on the réseau itself and that would lead to error.

I do not know whether I quite understand Prof. Turner's point, but with regard to the printing of the réseau, all that is done is that the plate in contact with the réseau is exposed to light and then it is afterwards exposed to the sky. But nothing is done between, no chemical process or development takes place, and it is difficult to conceive how the mere fact of exposing a plate for a short time in a réseau-frame should cause shrinkage.

Prof. Turner. If I may give an illustration of what I mean, without committing myself to the likelihood of its being a true explanation:—Supposing there were changes produced by temperature and not by development at all, the temperature of exposure to the stars would be different from that for the réseau, and thus the state of strain on the film might be different in the two cases.

The President. Prof. Dyson has sent us a short paper on the determination of the latitude of the Royal Observatory, Edinburgh, but as he is not present I will ask Dr. Halm to tell us about it.

Dr. Halm. I will just say a few words of the material from which the latitude has been derived and the general results. The latitude of the Royal Observatory, Edinburgh, hitherto adopted is 55° 55′ 28″·0 N. This was derived from a single observation made in 1889 at a time before the erection of the Observatory, by means of a reversible transit mounted in the prime vertical, and little value can be attached to this observation. From 1898 onwards two observers were engaged in the observation of zodiacal catalogue stars in accordance with Sir David Gill's suggestion, and from this material the present value of the latitude has been derived by comparison of the Edinburgh declinations with those given in Newcomb's 'Fundamental Catalogue' and with the 'Washington Catalogue of Zodiacal Stars.' In the first reduction, Bessel's refractions were used, and no correction for flexure was applied, but the comparison with the standard declinations showed at once that these assumptions were incorrect, so that a re-computation was made, using the Poulkova formula for refraction, and applying the observed flexure, which gave a more satisfactory accordance throughout. The value now derived is 55° 55′ 30″·0. It is rather curious that the latitude of our Observatory can be represented by writing five consecutive fives.

After a vote of thanks had been accorded for the paper, the President asked the Astronomer Royal to say something about the note presented by him on the determination of the wire-intervals of a transit-instrument.

Sir William Christie. I am afraid that Sir David Gill, in his comment on my previous paper, has somewhat anticipated what I have to say about this. It occurred to me, as it has to him, that in the determination of wire-intervals the main thing aimed at is not the distance of each wire from the centre wire, but of the mean wire from the centre wire, and that this can be effected easily in the case of a system of wires—9, say—which are symmetrically, or nearly symmetrically, placed with respect to the centre wire by the following method. Using a mercury basin, and pointing the telescope to the nadir, make wire 1 coincide with the reflected image of wire 9, and then wire 9 to coincide with the reflected image of wire 1: between these operations make the centre wire coincide with its reflected image, and so on with each pair of wires. The mean of the micrometer readings will give the position of the mean of wires 1 and 9, so that the distance of this mean from the centre wire can be easily found by subtraction. Similarly for each pair of wires, and the distance between the mean wire and the centre wire follows. The advantage proposed is that only the use of a very small part of the micrometer-screw is necessary.

Sir David Gill. A very ingenious plan, and I do not think I have yet seen it in print.

Prof. A. Fowler gave an account, with diagrams of spectra on the screen, of two papers by him already printed in the number of the 'Monthly Notices' for December 1906: one on "The Enhanced Lines of Iron in the Region F to C"—"there is abundant evidence that all the enhanced lines of iron in the region F to C are weakened in the spectra of sun-spots"; and the other on "Silicon in the Chromosphere," the existence of which Prof. Fowler now considers proved by identification of two red lines which are to be attributed to this element.

Mr. Newall. We are fortunate in having Prof. Fowler investigating this point about enhanced lines. The subject is a very important one, and it is made the more interesting because there is not complete agreement between observers. I may say that I have repeated the experiments of Hartmann in a preliminary manner, hoping to get definite information on the same lines as Prof. Fowler, but I must say the observations tended to support Prof. Hartmann, and so I look forward with interest to the result of further experiment. I do not feel quite inclined to agree with the last remark made by Prof. Fowler. I am sceptical as to the high-temperature explanation of enhanced lines. There seems as strong reason to think they may be due to a metal independent of the iron altogether, a metal brought into a special state by special conditions—electrical perhaps,—as that they are real iron-lines brought out under temperature conditions. I know it is an appeal to the unknown. One may venture to think, however, that electrical causes may be quite as active as temperature causes.

The President. I hope we may have an opportunity at some future date of hearing Mr. Newall describe his researches in this direction, so that we may hear Prof. Fowler's criticism of Mr. Newall.

Mr. Crommelin (presenting a paper by *Mr. Cowell* and himself on the perturbations of Halley's Comet). When I read a paper on Halley's Comet at the last Meeting I did not contemplate undertaking this work myself. In fact it is much too big a work for me to have undertaken, but I managed to get Mr. Cowell to undertake the work, and I have been able to help him in the numerical computation. We have now made a preliminary study of Jupiter's perturbations. We divided the orbit into 80 instead of 400 parts, and followed Pontécoulant's methods throughout with certain modifications, simplifying the work considerably without loss of accuracy, and we got as results the fact that perihelion passage will happen a fortnight earlier than M. Pontécoulant's date, that is, rather before the middle of May 1910; on the other hand, the suspicion expressed in December with regard to Pontécoulant's value of the eccentricity in 1910 is quite substantiated—in fact his eccentricity is quite wrong; his perturbations are at least ten times too great if not more, and the result is, instead of the perihelion distance shifting 9 million miles in the direction of the Earth's orbit, it remains practically the same as at the last return (0·59 astronomical units). We are now proceeding to a more detailed examination of the perturbations, using 180 divisions of the orbit. We think that is enough to get the perturbations with all necessary accuracy; in fact probably the second order terms, which no one has taken into account, would produce as great or even slightly greater errors than those arising from the use of only 180 divisions instead of 400. We are also examining perturbations produced by Venus, the Earth, and Neptune. And there is a point that may be mentioned here; it is the perturbations by Venus and the Earth produced in 1835 that we have to consider, not those pro uced in 1910; the perturbations they produce on the comet in 1910 will only affect the following return in 1986. The way in which these small planets affect the comet's return is by the slight difference they make in the comet's mean motion; the effect of this is insensible in the few days intervening between the approach of the comet and the perihelion passage, but in 75 years that small difference accumulates to several days; probably the Earth's effect makes the next passage 18 days earlier than Pontécoulant's value. So that everything tends to show that the return will be before the middle of May 1910. Perhaps I may mention that, according to Dr. Downing, an Indian mathematician is also doing these perturbations. I am very glad to hear that, because it is very easy to make mistakes, and therefore the more verification there is the better.

Dr. Downing gave, in detail, the results of a comparison of the places of stars in the Catalogue of Zodiacal Stars published by the American N. A. Office with the places of the same stars as given in the Cape General Catalogue. Good agreement was shown, except in the case of the Right Ascension of the fainter stars, where a systematic difference appeared, evidently due to the application of magnitude equation to the Cape observations.

The following papers were announced and partly read:—

John Tebbutt. "Observation of the Occultation of Saturn by the Moon, 1906 October 27."

A. M. W. Downing. "The Places of Zodiacal Stars for the Epoch 1900."

P. H. Cowell and A. C. D. Crommelin. "The Perturbations of Halley's Comet."

Rev. T. E. Espin. "Micrometrical Measures of Double Stars."

Rev. T. E. Espin. "New Double Stars."

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon made in the Year 1906." (Communicated by the *Astronomer Royal.*)

"The Latitude of the Royal Observatory, Edinburgh." (Communicated by *Prof. F. W. Dyson.*)

"Mean Areas and Heliographic Latitudes of Sun-spots in the

Year 1905, deduced from Photographs taken at the Royal Observatory, Greenwich: at Dehra Dûn; at Kodiakánal Observatory, India; and in Mauritius." (Communicated by the *Astronomer Royal.*)

Sir W. H. M. Christie, A. S. Eddington, and C. Davidson. "On the Errors of a Photographed Réseau."

Sir W. H. M. Christie. "Note on the Determination of the Wire-Intervals of a Transit-Instrument."

The following gentlemen were elected Fellows of the Society:—

Robert Boyce, Soudan Survey Department, c/o Royal Colonial Institute, Northumberland Avenue, London, S. W.

Arthur Cleminson, Deputy Commissioner of Lands, Lagos, West Africa.

George Innes, M.P.S., Olive Bank, Edinburgh.

Arthur Kent Lucke, Suez Canal Company's Service, Transit Department, Ismaïlia, Egypt.

George Street, M.A., Merton House, Southwick, Sussex.

The following Candidates were proposed for election as Fellows of the Society:—

Edward George Bloomfield Barlow, Ditton Lodge, Stourwood Avenue, Bournemouth (proposed by Col. E. E. Markwick).

Lieut. F. G. Cooper, R.N.R., H.M.S. 'Ocean,' 131 Sutton Court, Chiswick, W. (proposed by E. W. Owens).

Edward Power, F.S.A., F.G.S., 16 Southwell Gardens, S.W., and Watership, Newbury, Berks (proposed by W. S. Franks).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Sion College, Victoria Embankment.

Wednesday, 1906 December 19.

F. W. Levander, *President*, in the Chair.

Secretary: J. A. Hardcastle.

Mr. Hardcastle read the Minutes of the previous Meeting, which were confirmed and signed. He also read the list of presents received, and the thanks of the Association were accorded to the respective donors.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of six new Members was unanimously confirmed.

Mr. Hardcastle read a paper on "Astronomy without Instruments." He remarked that the writers of books on elementary astronomy spent much of their time in describing to beginners what they *would* see if they were to use a first-class telescope, or what they *would* detect if, after a great deal of preliminary practice, they were to use a delicate micrometer or a chronograph. He thought, therefore, it would not be out of place to show beginners who had no instruments what observations and measurements were within their power, and in particular how to make use of their neighbour's roof to detect the various movements of the Earth. If a mark were made on a window-pane so that all observations were made from a fixed point, the rising or setting of stars on an opposite roof could be observed with considerable accuracy. Mr. Hardcastle then gave an account of how a lady had discovered the effects of Precession, by this seemingly rough method, from the observed change in the relative positions of Sirius and δ Capricorni since 1898. Similar methods might be used to rate a clock and to keep true time, when once obtained, to within two or three seconds.

Mr. Heath said that a neighbour of his had recently shown him a device which he employed to get fairly accurate time. He had drawn a curve on a large scale on the ceiling of his bed-room, and on the window-sill he had a reflector consisting of an egg-cup with a little water in it. With this his friend showed him a transit of the Sun's image across the meridian line, the image on the ceiling being very clear and definite.

Mr. Crommelin said it was astonishing to find that a thing like precession, which was not discovered until the study of astronomy had been going on for several centuries, could be discovered independently after a few years of naked-eye observations. One could hardly have credited it, if it had not been actually done, as Mr. Hardcastle had told them. Mr. Crommelin went on to say that a good many Members of the Association were interested in observing occultations. If Members could observe occultations to the nearest second they would be doing really valuable astronomical work. There were still outstanding errors in the predicted places of the Moon. In fact, in the mean of the present year the Moon was six seconds of arc from the predicted place given in the 'Nautical Almanac.' Six seconds was a quantity that the Moon took some 12 seconds of time to describe, so that it was obviously of great value if one could give the time of an occultation to the nearest second.

Mr. Maunder remarked that Mr. Hardcastle had given the Meeting quite a charming paper. He did not think they could press too much upon young beginners in astronomy the usefulness and advantage of taking naked-eye observations of the apparent motions of the stars. But the chief interest of the paper was to him (the speaker) that it illustrated so well the conditions under which the earliest workers in astronomy had to make their observations.

Mr. Lynn said that Mr. Hardcastle's paper on observing without an instrument reminded one of that interesting episode in astronomical history when, in the early days of telescopic observation, Hevelius maintained that observations of place could be made *more* accurately by plain sights without a telescope; and Halley, afterwards Astronomer Royal, was deputed by the Royal Society to visit him at Danzig, the result being a report highly favourable to the accuracy obtained by Hevelius. That was in 1679, 70 years after a telescope had been first applied to the heavens by Galileo.

Mr. Hardcastle said that perhaps be might be allowed to emphasize one more point with regard to the method he had described, viz., that it required no adjustments of any sort. Each star was taken across any landmark it happened to go past. They need not fix their sights in the direction of N. and S. They could take any star in any direction at any moment when passing some landmark.

In reply to *Mrs. Arthur Brook*, *Mr. Hardcastle* said he did not think an observer using the method he had described would experience any disadvantages in observing through the glass of the window.

The President then asked Mr. Maunder to give his promised Address on "Greenwich Sun-spot Observations and some of their Results."

Mr. Maunder said that he had intended to take for the subject of his address, "Some Reasons why Greenwich Observatory should not be Moved," but a pressure of engagements had prevented his giving any time to the subject. The heliographic department proper of the observatory began its course at the end of 1873. The various instruments that had been used in it were: (*a*) the Kew photoheliograph of 3·6 inches aperture, (*b*) the Dallmeyer photoheliograph of 4 inches aperture—both these gave an image of the Sun 4 inches in diameter; (*c*) the Dallmeyer photoheliograph altered to give a solar image of 8 inches; (*d*) the Thompson photoheliograph, with an aperture of 9 inches, also giving an 8-inch image; (*e*) the Thompson 26-inch refractor, to which an enlarging-lens and camera were fitted, so as to take portions of the Sun's surface on a scale of 30 inches to the solar diameter. The full aperture was very seldom used with any of these instruments: the Dallmeyer was usually stopped down to 3 inches, the Thompson to 4 inches, and the great 26-inch refractor to 12 inches. The process originally used was that of the wet-plate collodion, but for many years past gelatine plates had been used, the kind mostly employed being very slow lantern-plates with hydrokinone development. The duration of the exposure in summer-time was equivalent to the one-three-thousandth part of a second. A glass plate (ruled with a reticule consisting of lines each one a hundredth of an inch apart, so as to break up the whole surface into squares each one-hundredth of an inch in side) was used in measuring the photographs. One of these squares corresponded very nearly to one-millionth of the whole surface of the Sun's hemisphere as depicted on the photograph, and that corresponded very nearly to one million square miles on the surface of the Sun itself. Enormous as was the size of some of the sun-spots as compared with the Earth, they were insignificant as compared with the Sun itself. Thus the superficial area of the great sun-spot group of 1892 was 18 times as great as that of the entire Earth, yet the loss of light would probably scarcely amount to one part in a thousand. It would therefore be seen that no great power of diminishing the solar radiation could be ascribed to sun-spots. The extraordinarily rapid variation in the spotted area of the Sun from time to time was another significant feature. The eleven-year period was familiar to all, but there were also rapid and extensive oscillations of the spotted area even at the time of maximum. And yet there was a certain stability about the areas of different types of spots. Regular or normal spots were limited in their size. There was very seldom a really well-defined circular spot of more than 600,000,000 square miles in area. They generally ranged from 150 to 300 millions of square miles. It had been much debated whether a sun-spot should be regarded as an elevation, a surface stain, or a depression. There could be no doubt that a photograph suggested the idea that they were looking into a hole, but the depressions, if they were such, must be so shallow in proportion to the area that a surface stain would best describe them. It was generally held that the darkness of the spots was due to absorption, but there was strong reason to believe that a large proportion of the darkness was due to lessened radiation, whatever might be the cause of such diminution. The rotation of spots was a subject frequently referred to, but, so far as his (the speaker's) recollection went, he had never seen a case in which there had been a certain and well-defined rotation of a pair of spots round each other. The proper motions of spots formed an interesting subject, for these were often remarkable. The determination of the rotation period of the Sun from sun-spots was a very complex work, but nevertheless there was an approximation to one mean period. When the entirety of all spots in any particular zone was taken it was found there was a marked tendency to a quicker rotation at the equator and a slower rotation near the poles, and that this change in the speed of rotation followed a simple law depending upon the second power of the sine of the latitude. Areas of special disturbance were shown by the tendency for spots to recur in the same region, and there was a marked tendency for sun-spot action to confine itself almost entirely to a few favoured regions, leaving the rest of the Sun almost wholly sterile.

Mr. Maunder at the close of his lecture showed some photographs illustrating how precisely the London County Council generating station at East Greenwich had been located on the meridian not only of the principal instrument of the Observatory—

the transit-circle,—but also on that of another instrument of importance, the altazimuth.

Col. Markwick presented an Interim Report of the Variable Star Section, which dealt more particularly with the peculiar light-variations of R Coronæ.

Mr. Gavin J. Burns exhibited a photograph, taken accidentally, of the trail of a meteor, which, he explained, seemed to have gone across the plate downwards.

Mr. Crommelin remarked that the photograph exhibited by Mr. Burns illustrated how very accurately photography was able to record the trail of a meteor—an accuracy, he supposed, 20 or 30 times as great as the best visual observation.

The President congratulated Mr. Burns on his good fortune in having secured so rare a photograph as that of the trail of a meteor.

The Meeting then adjourned at 7 o'clock.

ROYAL METEOROLOGICAL SOCIETY.

The Annual General Meeting of this Society was held on Wednesday evening, January 16, at the Institution of Civil Engineers, Great George Street, Westminster.

The President, on behalf of the Members of the Council, presented an Illuminated Address to *Mr. William Marriott*, in recognition of his services as Lecturer for the Society. The Address, which was signed by the individual Members of the Council, was as follows:—

> "The first course of lectures and exhibitions given on behalf of the Royal Meteorological Society having proved so popular as to lead to a further continuance, the Council of the Society avail themselves of the opportunity of placing on record their appreciation of the able manner in which Mr. William Marriott performed the duties of Lecturer and Demonstrator, which contributed greatly to the success of the scheme.
>
> "The Council desire at the same time to acknowledge the zeal and interest Mr. Marriott has shown at all times in the affairs of the Society, his many valuable contributions to the Journal and to the other publications of the Society during so many years, and also the courteous manner with which he has placed his knowledge and long experience at the service of observers in all parts of the country."

The Report of the Council for the past year, which showed the Society to be doing a considerable amount of useful work, having been adopted, the President, *Mr. Richard Bentley*, delivered an Address on "Weather in War Time," showing how greatly the course of history on upwards of three hundred occasions was influenced by weather conditions. Often invasions were frustrated or greatly impeded, from the time of the destruction of the fleet of Mardonius at Mount Athos, or of Xerxes off Eubœa, to the three successive Spanish Armadas threatening these islands, the Prince of Orange's descent upon England, or the French invasion of Ireland. Fogs also played an important part in history and were responsible for the deaths of such great men as Sir Philip Sidney at Zutphen, of the Swedish king, Gustavus Adolphus, at Lutzen, and of another veteran soldier, Marshal Keith, at Hoch Kirch. Still fogs were also turned to account. Lord Anson in a single ship loaded with Spanish treasure slipped unperceived through the entire French fleet, and the great Duke of Marlborough contrived to get his army across the Scheldt in the teeth of the enemy in a river mist. Reference was made to the difficulties overcome in crossing the snow on the Alps by Hannibal, Napoleon, Massena, Souvaroff, and Macdonald, and also to the march of Colonel Kelly's Sikhs to the relief of Chitral. The army of Frederick the Great was nearly snowed up at Mollwitz, and Marshall Wade's actually was so on one occasion in the Cheviots. Mention was also made of the hail-storm which smote Richard the First's men at Ascalon and of the sleet-storms at the battles of Hohenlinden and Eylau, and of the savage weather during the memorable retreat from Moscow. The important part played by ice was shown by the Swedish army on one occasion invading Denmark across the frozen Baltic, and by the Russians in 1809 similarly crossing over the frozen sea to Sweden; also the ice inclines in the passes prepared to hinder the Hungarians from crossing the Balkans, and the great ice forts built by the Austrians in the Tyrol, which could neither be clambered up nor shattered and were proof against all the French attacks. The capture of the Dutch fleet by cavalry galloping round the ships was due to the exceptionally severe winter of 1794–5, during which our troops in Flanders suffered so much. Another instance of ice in time of war was at the Lake of Tellnitz, which was the cause of a tragic episode in the great battle of Austerlitz. Instances of campaigns carried out at times of intense heat were given from the time of the Emperor Julian to the recent war in Manchuria—of troops perishing in dust-storms and of mirages in the desert. Some historic thunder-storms were also mentioned from the periods of Crecy and of Marston Moor, to those of the Peninsular War and at Quatre Bras. Even instances of earthquakes during battle were quoted, two of them in comparatively recent times. Wind was turned to account from early days by the Moors to surprise a fortress, down to the assault on the Royalists at Quiberon, and by its power of lowering the water for the capture of Stralsund in the Baltic, and Perekop in the Sea of Azof. Many battles, too, have been fought during storms, from the days of Malmesbury and Towton to those of Culloden and Solferino. Two great naval victories were gained in the height of the tempest, one by Lord Hawke and one by Lord Rodney. During one storm some unhappy nuns on board the fleet of Sir Thomas Arundell were thrown overboard by the superstitious sailors, but without averting the destruction of the vessels.

During the Meeting *Dr. H. R. Mill* was elected President for the ensuing year, and *Mr. F. C. Bayard* and *Mr. H. Mellish*, Secretaries.

The Planets and Planetary Observation.

[Continued from vol. xxix. p. 462.]

We now come to what we may term the modern or recent period of observation. On consulting the results obtained by many individuals we realize at once that it will be impossible to describe them with any detail, and it must be sufficient for our purpose to make brief references to, and extracts from, the accumulated mass of materials. It may be remarked, generally, that little substantial progress appears to have been effected in late years in regard to our knowledge of the forms, character, and motion of the markings, and that many striking differences exist in the work and conclusions of the various observers.

In 1871 Sir G. B. Airy mentioned at the R. A. S. Meeting on November 10 that, having laid down Bianchini's markings on a globe, he and Mr. Carpenter had looked for them without success. Mr. Lassell concurred, and Dr. Huggins said he had never been able to see anything like the markings of the early observers. Of course there were some at the terminator, and there seemed a difference of brightness near the convexity, but the appearances were delusive. Dr. De La Rue said he had many times seen markings on Venus and drawn them. They had reminded him of Mars, but were much less distinct. When the atmosphere was good he had generally seen them. Dr. Huggins mentioned that on that morning he had seen some irregularities, as if the surface were pitted with little craterlets like the Moon, and Mr. Browning stated he had noticed these and thought them real, but he failed to remark any large markings.

1871 May 24–June 14.—Vogel and Solise, at Bothcamp, observed the dark shading continuously just inside the bright limb.

In 1871 Mr. G. M. Seabroke, of the Rugby Observatory, using the 8¼-inch O.G. formerly in the possession of the Rev. W. R. Dawes, frequently saw markings on Venus. April 22, 8^h 15^m: three elongated markings seen running parallel to the planet's equator. May 1, 8^h 25^m: two dusky markings on E. part of disc. May 7, 7^h 55^m: one large cloud-like object extended over nearly one-half of the illuminated area of the disc running from N. to S., and most intense a little N. of the crater. May 18, 8^h: a dark feeble spot situated just E. of centre, fading away gradually. 8^h 15^m: a very peculiar large marking traced; it was somewhat like the letter X, and some parts of it were obvious nearly up to the terminator. On May 21 Mr. Henry Ormesher (5¼-inch O.G.) observed two small streaks, and the terminator was very irregular. In June the spots continued to be followed by several observers. On July 17, 6^h 30^m, Mr. H. W. Hollis (6-inch O.G., p. 150) saw the rounding-off of the S. cusp very distinctly, and the prolongation of the N. one was more remarkable than he ever before observed it. A dusky, ill-defined, and uncertain shaped spot was visible. On July 18 he suspected the presence of this spot again, somewhat nearer the terminator.

In August 1871 the Rev. T. W. Webb said the surface seemed in best moments clouded with feeble grey markings, but they were too faint to be distinctly made out.

In 1870, January 29, Mr. H. Pratt (8¼-in reflector, p. 180 and 270) records that several indentations in the terminator were remarked. Some delicate shadings were also perceptible (sketch in *Ast. Reg.* 1872 January).

In 1873 the writer, at Bristol (10-inch reflector), examined Venus on 15 afternoons. On several occasions a bright spot was seen near N. cusp. Very faint shadings were frequently seen when the atmosphere was steady, and especially on March 29, when a dusky band ran from the terminator between S. horn and centre to W. limb nearly. But the markings were not definitely bounded: they faded into the general tone of the surface, and therefore defied attempts to trace their forms, rate, and direction of motion. They could be described as faint grey appearances or centres of shading, and were nothing like the well-bounded spots on Mars or seas on the Moon.

Writing in 1873 Mr. N. E. Green remarked that he had frequently and earnestly looked for details, but had never seen more than a cloudiness about the equator and a slight increase of brightness near the poles.

At the same period Mr. Buffham pointed out that the markings were probably very variable. In April and May 1868 (with 9-in. reflector) he had frequently observed changeable details on the disc. On May 4, $7^h\ 30^m$, the surface was mottled with shady patches all over, while the terminator appeared speckled with dark and brighter points. Cusps sharp and brighter than other parts. May 6, about 7^h, a very faint shady streak seen near S. horn running obliquely from the terminator towards W. limb. On May 7, $8^h\ 30^m$, there were large cloudy spots all over the disc. On May 9, 7^h to 8^h, two dark spots were seen on the terminator, and faint cloudy patches distributed over the disc. June 5, $7\frac{1}{2}^h$ to $8\frac{1}{2}^h$: three darkish cloudy spots just within terminator. June 6, 7^h: the usual cloudy spots excessively faint. Bright streak from the N. cusp along the limb about 15°. Mr. Buffham remarked that it appeared to him marvellous that Mr. Dawes, "with every advantage of vision and of instruments, had never been able to detect anything more than a very doubtful blunting of the S. horn."

In 1868 Mr. With and the Rev. H. C. Key detected a bright prominence on the limb of Venus. The latter also saw granulations near the terminator, reminding him of the Moon's aspect.

1876 May and June. Mr. H. C. Russell (11½-in O.G. p. 200) reported a white patch seen at S. pole of Venus many times.

1877 Nov. 13 to 1882 Feb. 7. M. E. L. Trouvelot saw two remarkable white spots, reminding him of those on Mars; they were on the opposite limbs near the extremities of the cusps. The S. was brighter and became very prominent in 1878, January 16 to February 5, and appeared then to be composed of a multitude of bright peaks, forming on its N. border a row of brilliant star-like dots of light. Seen also by Seagrave at Providence with 8-inch O.G.

1881 Feb. to April. The writer at Bristol, 10-in. reflector, reported that faint dusky shadings, white areas, and bright spots near cusps were seen on the planet. The contour of the terminator, though not distinctly serrated, appeared undulatory, with sometimes a pretty obvious indentation. On April 5 the disc seemed to show many light and grey markings, with suspicions of minute bright specks near the terminator. The positions of the various markings at similar times on successive nights exhibited a slight westerly movement, approximately confirming Cassini's rotation period of about 23^{h} 21^{m}.

1884 April to May. Mr. A. Stanley Williams, 5¼-in. reflector, says faint dark spots were noticed and regarded as depressions. To the deepest of these a depth of 4 miles was ascribed, and to the ordinary spots about 2 miles. There was a dark band visible towards the south, which might represent a depression of 10 miles. A curious narrow streak just N. of S. horn and running parallel to the limb, was apparently permanent, and caused a small indentation of the terminator. This was suggested as being the same thing that gave Schröter the impression of a detached point at S. horn.

1884 March to May. On May 16, L. de Ball (10-in. Cooke O.G.) observed a dark spot on the terminator, and it extended far inwards towards the limb. The spot was crescented in shape and placed concentric with the crescent phase of Venus. On May 17 and 22 a large spot of approximately the same form was noticed between the terminator and apparent W. limb.

Prof. G. V. Schiaparelli, of Milan, published an important paper in 1890 on the rotation of Venus. He summarized his conclusions as follows:—

1. The rotation of Venus is very slow; it is made in such a manner that the position of the spots with respect to the terminator seems not to undergo any appreciable change during a whole month.
2. The rotation is probably accomplished in 224·7 days, that is in a period exactly equal to the duration of the sidereal revolution of Venus, and about an axis nearly perpendicular to the plane of the orbit.
3. Periods of six months at least, or of nine months at the most, might also agree with our observations, and a departure of the axis of rotation from the perpendicular reaching a limit comprised between 10° and 15° may be admitted.

4. Periods of rotation of about 24 hours must be completely excluded.
5. The rapid changes in the aspect of the planet, and especially of its horns, which are produced at intervals of about 24 hours, are the effect of the different conditions of vision from the varying altitude of the planet above the horizon and the different illumination of the background of the sky.
6. The observations of Bianchini are based on shadows which are too vague to lead to a certain period of rotation.
7. In the southern regions of the planet sometimes appear well-defined spots which seem to reappear from time to time with an identical aspect to lead to the idea of a casual relation with some stable formation fixed on the actual surface of Venus.
8. It is very important also to follow attentively certain small bright patches sometimes surrounded by dense shadows which now and then appear in pairs in different regions of the planet, more especially close to the terminator.

Schiaparelli based his conclusions on an exhaustive comparison of past observations and upon his own results obtained in the clear skies of Italy. Between 1877 November 5 and 1878 February 7 he made nearly 100 drawings of the planet's appearance.

1892. Trouvelot, as a result of twenty years' observations of Venus, concluded that there are vague grey spots which appear near the terminator and are of short duration. The brilliant spots on the limb are permanent and are very high mountains, occupying only the two regions where one ordinarily sees the horns. These mountains are so high as to extend above the dense atmosphere of the planet. The period of rotation is about 24 hours.

1890 May to Sept. Perrotin, at Nice, made a number of observations, which tended to show the stationary position of the spots and supported the long period of rotation given by Schiaparalli.

1892–93 and 1895. M. Mascari, at Catania, saw the markings retain a fixed position relatively to the limb and inferred that they corroborated Schiaparelli's period of rotation.

1894. M. Flammarion, at Juvisy, Paris, made observations in the years 1887 to 1894, and says there are polar snow-caps similar to those on Mars, but they are less evident owing to the difference in the general tone of the discs of Venus and Mars. The rotation period of Venus is not far from 24 hours.

1895, Nov. and Dec. Prof. Cerulli, at Teramo, could detect no diurnal changes in the markings, though he watched the planet during several hours at a time.

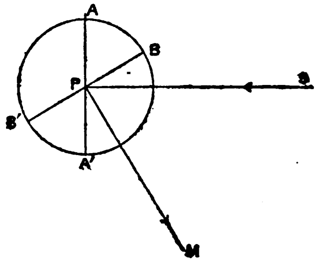
1895. Prof. Tacchini obtained observations in the last half of the year and found the features constant in position, thus confirming the period of 224·7 days.

Villiger, at Munich, examined the planet at the same epoch, and considered a short period, not differing much from 24 hours, the true one.

[To be continued.]

Brightness of a Planet.

In vol. xxix. pp. 388–90 I asked for information as to the method of estimating the brightness of a planet in relation to its phase, and, to simplify matters, assumed that the planet P was at a constant distance from the Sun S, and that the observer M moved in a circle round the planet, the plane of the circle lying in the plane SPM. The surface of the planet was also assumed to be uniform.



On pp. 462–4 appears further correspondence on this subject.

In commenting upon Mr. Badcock's letter I wrote very hurriedly and failed to notice a somewhat important error in his final formula for the brightness of P as seen from M. He uses the square of $\frac{\pi a^2}{2}(1+\cos\theta)$, but the formula should be

$$\frac{A}{R^2 r^2}\cdot\frac{\pi a^2}{2}(1+\cos\theta). \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

Here SP=R, MP=r, a is the radius of P, θ is the angle SPM, and A is the albedo.

Denote by U the area of that portion of the illuminated hemisphere of the planet visible from M, then (1) implies that the brightness of U varies directly with the area of U projected on to a plane BB′ through P, perpendicular to MP, this projected area being $\frac{\pi a^2}{2}(1+\cos\theta)$. My letter (pp. 463–4) must be read subject to this explanation.

Now $\frac{1+\cos\theta}{2}$ is the well-known factor used in estimating the phase of the Moon and the planets, and it is also implicitly involved in the *Nautical Almanac* formula employed for finding the dates of the maximum brilliancy of Venus.

But its use implies that the light reflected from the area U to the observer M is equivalent to that reflected from the projection of U on the plane BB′, and this is true only if U is self-luminous. The equivalence does not hold good when U shines simply by reflected light, as in the case of the Moon and the planets.

Dr. Spitta's experiments show this clearly for the condition when the angle SPM, or θ, is almost zero. The observer then sees practically the whole of the illuminated hemisphere, the planet being in full phase. But the light reflected to M from this hemisphere is decidedly less than would be reflected from a flat circular disc of the same diameter, placed with its plane perpendicular to SP. If we use Lambert's law, then the light reflected from the hemisphere towards M is only $\frac{2}{3}$ of that reflected from the disc towards M.

Lambert's law assumes that the diffused light is symmetrical about the normal, and is porportional to the cosine of the angle made with the normal by the direction of the diffused reflected light-rays.

Experiment shows that this law is only approximately correct, for, as the angle between the normal and the diffused ray increases, the intensity of the reflected light falls off more rapidly than the law indicates. Also it appears that, for equal changes in equal angles, the rate of diminution of intensity is different for different substances.

We are so accustomed in Optics to the law that the reflected and the incident rays are in one plane with, and on opposite sides of, the normal to the surface, and make equal angles with that normal, that it may be well to point out how completely different is the case for light diffused by irregular reflection.

In this case, for distant objects, the parallel incident rays make always equal angles with the parallel diffused rays; but the two sets of rays are not necessarily in one plane with the normal, nor are they necessarily on opposite sides of it, nor do they necessarily make equal angles with it.

An example may make this clearer. Suppose that SPM, or θ, the phase-angle, is equal to 60°, then the ray from S, incident on any point of the visible illuminated surface, makes an angle of 60° with the ray reflected from the same point to M.

But the angles between the normal and the incident rays, and those between the normal and the reflected rays, vary very greatly. In the plane SPM, let APA′ be a line perpendicular to SP, and BPB′ a line perpendicular to MP, and consider the angles of incidence (α) and those of reflection (β), made with the normal at different points of the arc BA′, which measures 120°.

The angle α at B is 30°, taken clockwise from the produced normal. This angle decreases to zero, and then increases up to 90° at A′, counter-clockwise from the normal.

The angle β at B is 90°, clockwise from the normal. This angle decreases to zero, and then increases to 30°, counter-clockwise from the normal. From $\alpha = 30°$ to $\alpha = 0°$ the incident and reflected rays are on the same side of the common normal; from $\alpha = 0°$ to $\alpha = 60°$ they are on opposite sides; and from $\alpha = 60°$ to $\alpha = 90°$ are again on the same side. Only at the point on this arc midway between B and A′ would the conditions of regular reflection be fulfilled.

For the points on the arc BA′ the rays are in one plane with the normal, but for other points on the visible illuminated area U matters are further complicated by the fact that the incident and the reflected rays are not in one plane with the normal.

As in the problem under consideration all the quantities (for a given phase-angle θ) are constant, except α and β, the quantity of light reflected to M from each point of U must be some function of these three angles, and by integration we shall obtain the total light reflected to M, and so be able to estimate the total brightness of U as seen from M. Unfortunately we do not yet know the exact form of this function for any one substance, nor do we know how it may vary for different materials.

In the case of the Moon, not only does the brightness fall off as we examine parts further from the subsolar point and nearer the terminator, but, owing to local differences in the nature of the surface-material, this falling-off is irregular.

In Mascart's 'Traité d'Optique,' tome iii., the subject of Photometry is pretty fully dealt with, but the particular problem of the influence of phase on the brightness of a planet is not specially considered.

For the total brightness of the visible portion of an illuminated hemispherical surface of uniform material Mascart gives the following formula, in which Lambert's law is assumed. Putting L for total brightness, then

$$\text{L varies as } \frac{2}{3}\left\{\frac{(\pi-\theta)\cos\theta+\sin\theta}{\pi}\right\} . \quad . \quad . \quad . \quad (2)$$

Here θ is the phase-angle SPM. If, for the sake of illustration, we represent by unity the value which L has, when it denotes the total brightness of the light reflected perpendicularly from a disc of radius equal to the hemisphere, when the incident falls also perpendicularly on this disc, then, putting $\theta = 0°$ in (2), we see that $L = \frac{2}{3}$.

Thus the hemisphere, in full phase, reflects only two-thirds of the light of the disc.

I have calculated the following table, and, in order to make the comparison more easy for different phases, I take L to be unity for the full phase, and express the values for other phases by fractions of this unit.

Of the planet's illuminated hemisphere, U denotes the area visible from M. The projection of this area upon a plane perpendicular to the sight-line is obtained by multiplying by $\frac{1+\cos\theta}{2}$ the area of a disc of the same radius as the planet. The last column of the table gives the mean intrinsic brightness of the projected visible area, and is obtained by dividing the total brightness value in the second column by the projected area in the third column, the final value in the last column being obtained by the method of limits. The second column is calculated by formula (2).

Phase-angle.	Total brightness of area U.	Projection of area U.	Mean intrinsic brightness of projected area.
0°	1·000	1·000	1·000
30	0·881	0·933	0·944
60	0·609	0·750	0·812
90	0·318	0·500	0·636
120	0·109	0·250	0·436
150	0·015	0·067	0·224
180	0·000	0·000	0·000

It will be noticed that the total brightness falls off more rapidly than the projected area, and still more rapidly than the intrinsic brightness, but it is, of course, the total brightness with which we are concerned in the case of a planet. By the above table the light of the half Moon is about $\frac{3}{10}$ of that of the full Moon.

If I understand the matter aright, the second column of the above table gives the relative total brightnesses corresponding to different phases, on the assumptions that the surface is uniform and that Lambert's law holds good.

To express the brightness in terms of that of the Sun, we must know the planet's size and its distances from the Sun and the observer. We must also know its albedo, by which I mean the constant-fraction expressing the reflective power of its surface as compared with that of a perfectly white surface, the two surfaces being similarly situated. (In my letter, p. 464, I used the term albedo in a different sense, as I was comparing the brightness of different parts of the same surface with each other, under different conditions.)

As Lambert's law is only approximately correct, and as the surface of a planet is not necessarily uniform, the foregoing results are only approximations to the truth.

My apology for the length of this letter must be the absence in English books of any full treatment of the subject. I hope that some one, much more competent than I am, will give us the benefit of his knowledge.

From Mascart's formula it seems clear that the maximum brilliancy of Venus will not occur just when the projection of her visible illuminated area is at its maximum for the Earth, though this is the usual supposition.

After I had written so far, Mr. W. E. Plummer very kindly drew my attention to Dr. Müller's profound German book on Stellar Photometry, which contains full details. A brief reference to this shows that Lambert's theory cannot be accepted, that Seeliger's theory is a good deal nearer the truth, but that only by an empirical formula, deduced from observation, can we really arrive at agreement.

For mean conditions, according to Müller, when Venus is at greatest brilliancy, the phase-angle, according to Lambert, would be 103° 47′; according to Seeliger, 116° 0′; and by the empirical formula, 118° 37′. By the *N. A.* formula it would be, I find, about 117° 56′, so that it seems as if, in this special maximum brilliancy case, we shall be nearly correct if, with the *N. A.*, we assume simply that the brightness varies as the projected visible area, and take no account of the obliquity of the rays.

Leeds, 1906, Dec. C. T. WHITMELL.

Problems of Solar Physics.

To the zeal and energy of Prof. George E. Hale is due the inception of an international union for the study of Solar Physics, which should include among its members both astronomers and physicists representative of the leading scientific academies of the world. The first meeting of the delegates, sixteen in number, from fourteen different academies of mixed nationalities, took place at St. Louis on September 23, 1904; the second, at which forty members were present twenty-three of whom were delegates from eight different nations, assembled at New College, Oxford, on September 27–29, 1905. The Transactions of these two meetings, with papers either read at the conferences or prepared in view of the conferences, together with the resolutions adopted and the rules for the constitution of the Union, have been gathered together in a valuable and handsome volume under the care of Prof. Schuster, of the University of Manchester, which appears with the title of Vol. I. of the Transactions of the International Union for Co-operation in Solar Research*. There are four main problems, or lines of research, which engaged the attention of the delegates present at the meetings, and which formed the subject-matter of the resolutions which were eventually adopted after ample discussion. These problems concern the determination of reliable standards of wave-length, the exact measurement of the intensity of solar radiation, the study of the spectra of sun-spots, and the study of the records, by means of the H and K light, of the phenomena of the solar atmosphere, while a related problem, namely that of the solar rotation as measured by spectroscopic methods, came up for review, without, however, leading to the formulating of any definite resolutions. The progress of physical science is indissolubly bound up with exactness in quantitative measurement; very fittingly therefore was the attention of the Union directed at the very outset to the subject of the value of the fundamental wave-lengths which constitute the basis of all exact observation in spectroscopy. It would at first sight have appeared that the immense labours of Rowland of Baltimore, which resulted in the construction of gratings of great purity, and a photographic map and tables of the solar spectrum giving the positions and intensities of some 20,000 lines, besides identifications with the arc-spectra of most of the known elements, would have sufficed for the needs of even the most exact spectroscopy for generations to come. But in an age when it is possible to measure radial velocities of stars to the hundredth of an Ångström unit, which corresponds to a line of sight velocity of 0·7 kilometres per second, tables of standard wave-lengths are required at least to this degree of accuracy; and although Rowland's Tables purport to be exact to three places of decimals, yet they have been found wanting in parts even to two. An interesting paper by Mr. Jewell gave the history of the construction of Rowland's map and tables. The fundamental wave-length upon which they depend is that of the D_1 line as determined by Bell, and the wave-lengths of the other lines were derived by the method of coincidences between the lines in different orders of the spectrum. But the fixing of the wave-length of the red cadmium line in terms of the standard metre by Michelsen by means of the interferometer, a result which has been independently confirmed with an instrument of somewhat different construction by Fabry and Pérot, virtually implies errors in the standard position of the lines measured with reference to D_1. The fault seems to lie in the gratings themselves, which have proved to be not sufficiently accurate instruments for the determination of absolute wave-lengths. Mr. Jewell contended for the re-measurement of Rowland's best series of plates, and the re-determination of relative wave-lengths from them, and naturally deprecated haste in setting aside Rowland's gigantic work. Hartmann also would patch up Rowland's Tables. The conference at Oxford determined, however, that a suitable spectroscopic line shall be taken as the primary standard of wave-length, which shall be fixed permanently and thereby define all other wave-lengths and their unit. This unit, which is to be called the Ångström, shall differ as little as possible from 10^{-10} metres. Secondary standards are to be at distances apart of 50 Ångströms, also determined by interferometer methods, while tertiary standards at distances of 5 to 10 Ångströms are to be interpolated by the use of gratings.

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* Manchester: at the University Press, 1906. Pp. xii and 257. Published by Messrs. Shenatt & Hughes. Price 7*s*. 6*d*. net.

The subject of the solar emission of energy and the exact determination of the "solar-constant" was treated of in papers by several physicists, and in particular in a lengthy and most valuable review of the whole subject by Prof. Abbot. One interesting point touched upon was the actual variability of the solar radiation in itself, independently of that due to meteorological conditions. And here it is well to mention that the solar physicists join hands with the meteorologists, and that the solar Commission of the International Meteorological Committee was represented among the associated academies. But to return to the intrinsic variability of the solar radiation itself. It was suggested by Prof. Julius that this was due to unequal distribution in various directions, which was conditioned by inequalities in the Sun's field of radiation. This would result from ray-curving in the strata and vortices of the solar gases, the order of magnitude depending on the refractive indices of the various gases. Although experiments with all forms of apparatus for registering the intensity of solar radiation were encouraged, especially ingenious pieces of mechanism being described by Prof. Abbot and Prof. Callendar, the standard instrument adopted by the Union was the electrically controlled compensating pyrheliometer of Prof. K. Ångström. The Union, moreover, resolved that for the determination of possible changes in the solar-radiation power, measurements of radiative intensity should be secured over limited ranges of the spectrum which are not affected by absorption due to constituents of the atmosphere such as ozone, water-vapour, and carbonic acid.

In his presidential address to the first Conference at St. Louis, Prof. Hale insisted upon the necessity of a fuller knowledge of the spectra of sun-spots. One of the committees of the Union has charge of the observations in this important branch of solar physics, and of this committee Prof. Alfred Fowler is the active secretary.

In the present volume appears a noteworthy paper by Prof. Fowler, with a table and a map, which is to serve as a model for the presentation of their results by other observers. The observations were made during fifteen months in the years 1905–6, and cover the part of the spectrum included between the lines b_4 and E_1. Treating of the bands which are to be found scattered over the general darkening of the continuous spectrum due to the spots, Prof. Fowler calls attention to the structure of these bands, which resemble the fluted spectrum of chemical compounds, and the bright gaps of separation in the general darkening which are generally found to extend over the photosphere and are bounded on each side by lines of the ordinary Fraunhofer spectrum which have a faint and hazy appearance. The inference is that the material which gives rise to the spectrum of bands in the general darkening due to a spot, while concentrated in the spots, is scattered thinly over the entire photosphere. The origin of this spectrum has so far eluded detection. Coming now to the lines which are affected in the spots, Prof. Fowler finds in the first place that the selective widening does not differ from spot to spot, but is constant. This position, at least as far as regards the red end of the spectrum, has been substantially insisted upon in several papers by the present writer during the last twenty years *. An important point dealt with by Prof. Fowler is that of the lines which are weakened or obliterated in sun-spots. These are found to be high-level chromosphere lines, and moreover are precisely those lines which are enhanced in passing from the arc to the spark. When there is added to this fact the further facts which appear in the discussion, that for chromium and iron the relative order of intensification in spots corresponds to that of the arc or solar spectrum, and further that in the case of iron and vanadium, this element being one of the chief constituents of sun-spots, the lines especially intensified are those characteristic of the arc-flame spectrum, the conclusion that the additional selective absorption in sun-spots is produced by relatively cool vapours seems to be a legitimate one. In a word, the darkness of a spot is due to absorption caused by a lowering of temperature. This view corresponds with that of Prof. Hale and his coadjutors, as was so admirably set forth in the January number of the *Observatory* by the late lamented Miss Agnes Clerke. Although in point of time of actual publication the American results were first in the field, we happen to know that Prof. Fowler's paper has been awaiting publication for a considerable period. That two lines of research upon the spectra of sun-spots independently undertaken, the one visual and the other photographic, should have led to the same general conclusion is worthy of note. At the same time it is only fair to state that such authorities as Mr. Evershed and Mr. Maunder hold that the darkening of a sun-spot is due to a diminution of radiation. The spots are, on this view, really hotter than the photosphere, the photospheric clouds having been evaporated by a local access of heat. It is difficult, however, to reconcile this view with the existence of spot-bands, which, as pointed out by Prof. Young and the present writer long ago, are indicative of such a lowering of temperature over the regions of sun-spots as to allow of the formation of chemical compounds. A. L. CORTIE.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Discovery of Vesta.

GENTLEMEN,—

It will, next March, be exactly one hundred years since Olbers discovered Vesta, which for nearly thirty years seemed to have completed the number of small planets revolving between the orbits of Mars and Jupiter. It was through his suggestion that other bodies would be found passing near the intersection of the orbits of Ceres and Pallas that Harding discovered Juno (in 1804) and he himself Vesta in 1807. Juno is the smallest of the four; Vesta is the brightest and was, for that reason, formerly thought to be the largest; but Prof. Barnard's measurements with the 36-inch Lick and 40-inch Yerkes telescopes, which were published in the 'Monthly Notices' in 1900, show that Ceres is the largest and that the superior brilliancy of Vesta is due to the greatness of her albedo, which is difficult to explain, and renders her the most interesting of the small planets, always, of course, excepting Eros, which owes its interest to a very different cause not connected with its own nature. The inclination and eccentricity of the orbit of Vesta are not great; the former very slightly exceeds that of Mercury, and the latter is somewhat less than that of Mars. She will come into close conjunction with the Moon several times during the present year; but, unfortunately, these will all take place during daylight. She will be in conjunction with the Sun on the 10th of May and no opposition will occur until the 4th of January, 1908, so that next winter will be a favourable time for examination of her surface.

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* *E. g.* M. N. R. A. S. vol. xlix. no. 8, p. 417, June 1889.

Olbers announced his discovery of this planet on the 29th of March to Schröter in a letter dated the 31st, in which he says that the planet was where he expected to find one, in the northern wing of Virgo; brighter than 20, but not so bright as ρ Virginis. He requested him to communicate the news of the discovery to Bessel, who was then on a visit to Lilienthal; and in a subsequent letter expressed his opinion of the probability that the Parisian astronomers might have anticipated him, and still more likely that Harding had discovered the planet almost simultaneously, as he had recently called his attention to that part of the sky as one where a small planet might be found. However, he remained the sole discoverer—the first to discover two primary planets of the solar system. What would he have thought of the number recently detected by Dr. Palisa and others, not to mention the photographic discoveries at Heidelberg! The first mention I can find of the name Vesta is in a letter from Bessel to Olbers, dated Lilienthal, 27th April, 1807, which begins "Hier erhalten Sie zwei Beobachtungen Ihrer Vesta . . . . .," so that allusion is probably made to a letter which is not published.

Olbers continued his search, but detected no more; he died in 1840. Some years before that time, Hencke had begun his long search for planets, which did not bear fruit until nearly the end of 1845, when he found Astræa, a discovery which the astronomical world were discussing when the investigations were in progress which led to a successful search for a much greater exterior planet by its perturbing effects upon Uranus. Hencke discovered only one more small planet (Hebe) in 1847; somewhat later in that year Hind's discoveries began. It is not thought worth mentioning in many astronomical books that Cacciatore, who succeeded Piazzi at Palermo in 1826, discovered a small planet ten years afterwards, but the observations made were not sufficient to keep it in view or determine its orbit. This was nearly thirty years after the discovery of Vesta. Yours faithfully,

Blackheath, 1907, Jan. 4. W. T. LYNN.

OBSERVATORIES.

TORTOSA.—This name is as yet unfamiliar in these pages, in fact there was some doubt whether the above name is the appropriate heading to this paragraph, which relates to an institution whose postal address is Observatorio del Ebro, Tortosa, Spain, and whose first volume of Memoirs has kindly been sent us. The Observatory, under the direction of Father R. Cirera, S.J., is attached to a college of Jesuit Fathers at Tortosa, near the east coast of Spain, and, judging from the book before us, is handsomely equipped and may be reckoned on making valuable contributions to the study of cosmical physics in the future. The memoir is, in the first place, an account of observations of the total solar eclipse of 1905 August, for Tortosa had the advantage of being within the path of the shadow, and for this reason the Observatory found itself a centre of much attention from astronomers of many countries. But beyond saying that there is a very beautiful drawing of the eclipsed Sun, some records of terrestrial magnetism on 1905 August 30, and some photographs of the corona, nothing more need be said on this subject, especially as other volumes of details are promised later. The Observatory has been established, as is learned from the first chapter, to furnish a great number of data for the solution of the problem of the relations which exist between the solar activity and several phenomena which are observed on our planet, especially the electric and magnetic phenomena. To which end it has a twin-telescope for photography and eye observation equatorially mounted, a spectroheliograph, after the plan of Mr. Evershed, for photographing the Sun's surface with spots and flocculi, in calcium or some other light, and a spectrograph for visual observation of the prominences. There is also installed a complete apparatus for registering Earth-magnetism, with meteorological and seismological instruments. Considerable care was taken in the selection of this equipment before the institution was founded, and the Director visited many other observatories to gain information as to the most suitable form of instruments. Also it may be remembered that Father Cirera attended the International Solar Conference at Oxford in 1905, so that he is quite alive to the present needs in solar physical research. We offer Tortosa our best wishes for its future career.

The following reports of American Observatories, which were also sent to the Astronomische Gesellschaft, are in continuation of the series on p. 66.

Chamberlin Observatory at Denver. *A. H. Howe.*—Observations of comets visible in 1905. Experiments on special methods of noting time and certain varieties of personal equation.

Flagstaff (Arizona). *Percival Lowell.*—Observations of Mars before and after opposition (May 8), visual observations being supplemented by photography. A large number of photographs show the canals and other surface-markings. The photographs were taken with the 24-inch Clark refractor, employing Prof. Ritchey's colour-screen method, the negative amplifying-lens having an equivalent focal length of 148 feet. Mr. Slipher has continued his photographic spectra of the planets in the less refrangible region of the spectrum for the investigation of their atmospheres. The results for Jupiter, Uranus, and Neptune have been already published. Work has been done on the radial velocity of Standard Velocity Stars (see *Astrophysical Journal*, vol. xxii.). Photographs of comets.

PUBLICATIONS.

Cape Publications.—The Cape General Catalogue 1900 has recently been issued, a volume which contains the places of 4464 stars reduced to the equinox 1900·0, comprised in four sections. The first two are catalogues deduced from observations with the non-reversible transit-circle, one of them containing the places of 2798 stars observed in accordance with a resolution passed at a conference held at Paris in 1896, to the effect that it would be expedient to have a fundamental catalogue of stars covering the zodiac, which could be used for differential observations of planets or the Moon made with the heliometer or by other differential methods. The second section contains stars south of the Cape zenith wanted for various purposes. Catalogue III. gives the places of 63 stars from equatorial observations, and Catalogue IV. is a list of 41 stars with their places derived from photographic plates.

With the above were distributed two other volumes of 'Annals of the Cape Observatory.' One gives the details and results of Mr. Bryan Cookson's determinations of the mass of Jupiter and orbits of the satellites, of which a summary was published in the 'Monthly Notices R. A. S.' 1904. The special point of Mr. Cookson's method was that he observed the mutual distance and position-angle of each pair of satellites instead of measuring the positions of the satellites from the limbs of the planet—an obviously unsatisfactory method. From these measures he deduced the mass of Jupiter, the compression and the elements of the orbits of the four satellites. The other volume is on a very cognate subject, being a determination of the inclination and nodes of the orbits of Jupiter's satellites determined from photographs taken at the Cape Observatory. It will be quite understood that it is not possible to give more than this brief mention here of these works because of their technicality, but they are doubtless valuable contributions to the Jovian theory.

NOTES.

Obituary.—Agnes Mary Clerke. We of this magazine, though much grieved, were not surprised to hear of Miss Clerke's death, for having sent to her the proof of her excellent article in last month's number, it was returned uncorrected by a relative, with a note to say that she was lying in a serious condition, as the result of pueumonia following influenza. She died on Sunday, January 20.

Miss Clerke, born in Ireland on February 10, 1842, was the daughter of the late John William Clerke. She lived in Italy in the years 1870–77, and in the latter year began writing for the *Edinburgh Review*; but the work that brought her to the notice of the astronomical world was her 'Popular History of Astronomy during the 19th Century,' published in 1885, which was intended to carry on to date the scheme of Prof. Grant's invaluable book, and at the same time to embody the new astronomy which had grown up since the advent of the spectroscope as an astronomical instrument. It was at once apparent that there had arisen a powerful writer who was not only able to collate and compile facts, but was able to discuss and discriminate, and since that time Miss Clerke has been recognized as one whose opinion on astronomical subjects was of the highest value. Besides the book already mentioned, which reached a fourth edition in 1902, other well-known works by her are: 'The System of the Stars,' published in 1890, in which she considered the evolution of stars of all kinds, the translation of the heavens and their construction; and 'Problems in Astrophysics,' published in 1903, which, besides being a history of the researches in physical astronomy, suggested lines of thought and future work. Miss Clerke had a wonderful command of language and her books were models of literary as well as scientific skill. As a reviewer said of one of her works, they were not "popular," in the sense that she did not minister to people who are too lazy to do more than attend scientific lectures to have their ears tickled, but her books ministered to those who long to know and who do not grudge the best intellectual exertion they are capable of. Her smaller efforts were 'The Herschels and Modern Astronomy,' 'Astronomy' in Concise Knowledge Series, and frequent articles in this Magazine, in which she was good enough to give our readers the benefit of her opinion on points in current physical astronomy. She also wrote articles in the 'Dictionary of Natural Biography' and in the 'Encyclopædia Britannica,' her contribution to the latter work on Laplace being specially noteworthy. In 1892 she was awarded the Actonian Prize of one hundred guineas for her works on astronomy, and in 1903 she was elected with Lady Huggins an Honorary Member of the Royal Astronomical Society, a title, held previously only by Miss Caroline Herschel and Mrs. Somerville, which was resuscitated to honour these two ladies. Miss Agnes Clerke with her sister, Miss Ellen Clerke, who predeceased her, were frequent attendants at the meetings of the Royal Astronomical Society, and she has sometimes spoken on subjects dealing with her special branch of astronomy. She will be missed at Burlington House, and we cannot but feel that this Magazine has lost a contributor whose place will not easily be filled.

PROF. OUDEMANS. We have also to record with regret the death of this astronomer, Director of the Utrecht Observatory, which happened on December 14 last.

Jean Abraham Chrétien Oudemans was born at Amsterdam on December 16, 1827. His later education was obtained at Leiden, and in 1856 he was made Professor of the University of Utrecht and Director of the Observatory of that place. Later he was made chief engineer of the geographical service of the Dutch East Indies, and much of the work of his life was on subjects connected with surveys, his report on the Triangulation of Java being a voluminous work published in six volumes.

Oudemans accompanied Prof. E. F. Bakhuyzen and Prof. Kaiser to the Isle of Reunion to observe the Transit of Venus, 1874; the account of the determinations of longitude of the phase and some remarks on the observation of the Transit, which appear to have been not especially successful, were published only a year or two ago. He also published various other works of a miscellaneous astronomical nature, the last being two papers in the Proceedings of the Amsterdam Academy of Science of October and November, 1906, on the mutual occultations and eclipses of Jupiter's satellites which will happen in 1908.

WILLIAM SIMMS. The Senior Fellow of the Royal Astronomical Society was Mr. William Simms, formerly a member of the well-known firm of Troughton and Simms, and brother of Mr. James Simms, the present head of the business. He was elected on 1851 January 10 and contributed several papers to the *Monthly Notices*, the last being a note in the number for June 1874, on a paper on the Dispersion Spectrum that the present Astronomer Royal had previously contributed. Mr. Simms died in the ninetieth year of his age, on January 2.

E. E. McCLELLAN. This gentleman, who died, we are sorry to say, on January 4, at the early age of 45, was third Assistant at the Radcliffe Observatory, Oxford, having held the post for 14 years. He had previously served for 12 years on the staff of the Magnetic and Meteorological Department at Greenwich.

Mr. McClellan joined the staff of the Radcliffe Observatory on 1892 Sept. 19, and was at first entrusted with the general care of the Meteorological Department, for which his previous experience had eminently fitted him, but he soon qualified himself for participating in almost all the operations of the Observatory. As an astronomical observer he shared largely in the observations made with the Transit-Circle recently published in the 'Radcliffe Catalogue of Stars, 1900,' and with the equatoreals he proved to be a capable and handy man. As he especially excelled in photographic skill, his services were in continuous requisition in connection with the new photographic telescope. It was in this department he displayed his greatest interest, and here his loss will be chiefly felt.

He was a great favourite with all the members of the staff, and his sudden death, from cerebral hæmorrhage, on January 21, at the comparatively early age of 45, has caused the deepest regret to all his colleagues. He leaves a widow, but had no children. W. W.

THE death is also announced of Mr. T. R. DALLMEYER, Managing Director of the firm of opticians which bears his name. He was the son of Mr. J. H. Dallmeyer, and grandson of Mr. Andrew Ross, both pioneers in the methods of modern optical work. He died somewhat suddenly at the early age of 47.

THE NEWALL OBSERVER.—The Syndicate who have the Cambridge Observatory in their charge recommend that an Assistant of University standing be appointed to aid Mr. Newall in his astrophysical and solar-physical researches. The appointment is to be made by Mr. Newall with the consent of the Vice-Chancellor, for a period of five years, but terminable with Mr. Newall's tenure of his office. The salary attached to the a sistantship is £200 per annum, half of which is generously provided by Mr. Newall.

A 100-INCH MIRROR.—Mr. J. D. Hooker has presented to the Carnegie Institution of Washington the sum of forty-five thousand dollars to purchase a glass disc 100 inches in diameter and 13 inches thick, to be made into a mirror of 50 feet focal length, and for expenses incidental thereto. This mirror is intended for use at the Mount Wilson Observatory, under the direction of Prof. Hale.

THE ECLIPSE OF THE SUN.—A Reuter telegram from Samarkand states that the eclipse of the Sun was observed from a point at the 1481st verst, on the railway between the stations of Kuropatkino and Mijulnskaja. The first sign of the eclipse was noticed shortly after 9 o'clock, and the faint shadow gradually grew darker until, at 9.53, the period of total eclipse set in, lasting for two minutes. The shadow then began to pass away, and shortly after 10 o'clock the Sun was clear. Throughout the time of observation snow was falling.

THE Secretary of State for the Colonies has appointed Major E. H. Hills, C.M.G., R.E., to inspect and report upon the survey departments now working in the Protectorates of British East Africa and Uganda. Major Hills left for Mombasa on January 9 last. On the completion of the above-mentioned work he will proceed to Colombo, to make a similar inspection in Ceylon.

MR. SYDNEY S. HOUGH, F.R.S., Chief Assistant in the Royal Observatory, Cape of Good Hope, has been appointed His Majesty's Astronomer at the Cape, on the retirement of Sir David Gill.

PROF. SCHUSTER has resigned the position of Langworthy Professor of Physics and Director of the Physical Laboratories in the Victoria University of Manchester, which he has held since 1888. He is to be succeeded by Prof. E. Rutherford, F.R.S., Macdonald Professor of Physics in the McGill University.

Prof. Schuster has offered to the University of Manchester an annual sum of £350 for three or four years as the stipend of a Reader in Mathematical Physics. He is evidently of opinion that experimental physics is being pursued to the neglect of the mathematical side, and that some encouragement of the latter will be to the advantage of science.

SIR CHARLES TODD has severed his last connection with official life by giving up the Directorship of the Government Observatory, Adelaide, South Australia, which he has held since its establishment. We offer him the heartiest wishes for health and long life to enjoy his retirement. Mr. Griffith, his Chief Assistant, will succeed him as Director of the Observatory.

THE Gold Medal of the Royal Astronomical Society is to be awarded by the Council to Prof. ERNEST BROWN, F.R.S., Professor of Applied Mathematics at Haverford College, Pennsylvania, U.S.A., for his researches in the Lunar Theory. Prof. Brown, though now holding an appointment in an American College, is an Englishman, and was formerly a Fellow of Christ's College, Cambridge.

THE Annual General Meeting of the Royal Astronomical Society will be held on Friday, February 8, at 5 o'clock in the afternoon. The next Meeting of the British Astronomical Association will be on Wednesday, February 27.

FROM AN OXFORD NOTE-BOOK.

THE death of Miss AGNES M. CLERKE is a real grief and a real loss to us all. This is not the place in which to refer to it at length, but to refrain from a word or two is impossible. The true magnitude of her achievements is, perhaps, scarcely realized except by the few who have attempted work of the same kind. Prof. Simon Newcomb, in the preface to his book on 'The Stars: a Study of the Universe,' frankly states that when he accepted the invitation to write it "he supposed that it would be an easy task to sketch in simple language, for the lay as well as the scientific reader, the wonderful advances of our generation in the knowledge of the fixed stars," and as frankly admits that he found the difficulties not to be surmounted in a way satisfactory to himself. "The problem was, now to study whole chapters of observations and researches on some minute branch of the subject, and condense their gist into a few sentences; now to search volumes of periodicals, perhaps in vain, to find who was first in some field, or what result some investigator had reached; now to do justice to the respective works of students of the same subject; now to recast or rewrite passages in the light of some newly published research." These words accurately represent the problem at which Miss Clerke worked untiringly for a quarter of a century with brilliant success. Doubtless there are passages in her books, perhaps even many such, which this or that reader may find fault with; but need this surprise us when Simon Newcomb declares at the end of his labours that the "professional reader will find defects that may seem to him serious in nearly every chapter"? Our mental vision must be indeed be-fogged if we cannot at this moment lose sight of the less brilliant passages in the splendour of the whole work well and nobly done.

A STRIKING series of articles on radioactivity have appeared in the *Athenæum* recently. A few sentences from that by Gustave le Bon on November 17 may illustrate their interest for astronomers:—

> Si on tient à admettre que la radio-actitvité est la cause de la température intérieure du globe, il n'est nul besoin d'invoquer la présence supposée du radium. Tous les corps incandescents, comme semblent l'être ceux existant dans l'intérieur du globe, dégagent des torrents de particules électriques qui suffiraient à produire l'effet supposé. Il serait fort possible qu'elles jouent un rôle, sinon dans le maintien de la chaleur du globe, au moins dans la production des tremblements de terre.
>
> . . . .
>
> Parmi les divers exemples de cette radio-activité artificielle je me bornerai à en citer deux. Le premier est celui de la radio-activité et de la phosphorescence qu'acquière le sulfate de quinine auquel on ajoute des traces de vapeur d'eau. . . . .
>
> Le second exemple que je citerai est choisi parce qu'il est d'une répétition extrêmement facile, si on veut bien suivre les indications que j'ai données ailleurs. Le mercure n'est pas radio-actif sous l'influence de la lumière; or il suffit de lui ajouter quelques millièmes de son poids d'étain pour qu'il devienne à surface égale quarante fois plus radio-actif que l'uranium.
>
> Donc jusq'au jour—fort lointain, je crois—où on aura isolé le radium, nous serons autorisé à dire que ce métal supposé n'est qu'une combinaison, de nature d'ailleurs inconnue, due à la présence en quantité tres minime d'une autre substance.

The very important suggestion is that there is no need for the presence of radium or other substance in bulk; minute quantities of something may produce an extraordinary effect on something else. Life itself may be one manifestation of the kind.

THE following letter addressed by a veteran astronomer to the *Windsor and Richmond (N.S.W.) Gazette* for November 24 seems worth reprinting here. During the visit of the British Association to York we had an opportunity of seeing the "Nature Study" note-books kept by the scholars of a famous school, with a very old-established natural history society. Choice of subject was apparently unrestricted, and there was consequently a luxuriant variety. The collection was most interesting and instructive, and we astronomers only regretted that, although a small but well-equipped observatory was attached to the school, there was only one astronomical note-book. Encouragement in the direction indicated by Mr. Tebbutt is apparently needed elsewhere than in Australia. Incidentally, may it be noted with satisfaction that he has not relinquished his own laborious and valuable astronomical work, as he half threatened? This is his letter:—

THE PROPOSED NATURE STUDY EXHIBITION AT RICHMOND.

(To the Editor.)

SIR,—On the 19th inst. I was officially informed that it was intended to have a Nature Study Exhibition at Richmond on the 23rd and 24th inst. for the benefit of the public schools of the Hawkesbury District, and I was at the same time asked to contribute towards the laudable object. Until this information reached me, which was rather late, I was not aware of the proposed exhibition. I observe, however, from the printed schedule of prizes, that the projectors of the exhibition, following the course uniformly adopted by our University, ignore that part of Nature which is outside of the Earth on which we dwell. In this country, unfortunately, the oldest and grandest of all nature studies is completely in the shade, and it is on this account that ignorance with regard to the mechanism of the universe is so strongly and painfully prevalent amongst us. Surely children can be as much interested and intellectually improved by a study of the heavens as they can be by an examination of the life history of the frog or of any insect. For my own part I shall have much pleasure in offering a prize in the form of some book on popular astronomy, so that I may in some measure make amends for the slight which has been offered by the committee to the oldest, grandest, and most comprehensive of all our forms of knowledge. To this end I will give a suitable prize for the best illustrated essay on any nature study topic. See No. 2 of Section VI. in the printed prize schedule. Let us hope that astronomy, as well as zoology, botany, and geology, will find a place among the competitive essays.—I am, Sir, yours faithfully, JOHN TEBBUTT.

The Observatory, Windsor, November 20, 1906.

FROM another Australian Colony comes the news that another veteran, though "still an active and clear-headed man," feels that "at 80 years of age it is time for him to retire." Sir Charles Todd also gives the reason that "he does not want to stand in the way of Mr. R. F. Griffith, who has been his right-hand man at the Observatory, in connection with the establishment of a Federal Meteorological Department." He has, of course, been asked "to what he attributed his mental and physical vigour at so great an age?" and his reply was:—"To a good constitution, I should think, to plenty of work, and a habit I have formed of not worrying myself over trifles. Nothing ages one so much as worry. If I have any official troubles I never take them home with me." Of such excellent precepts one can only say, with Captain Cuttle, "when found, make a note of." The proudest day in his long life Sir Charles gallantly declares to be that on which he was married, and the next in order that on which he "sat on the ground near Central Mount Stuart on a cold night and with a little pocket relay connected with the wires and communicated with Port Darwin and Adelaide." The establishment of telegraphic communication across thousands of miles of Australian desert is, in very truth, an achievement of which a man may well be proud; it is not to be done again.

It is recorded that not a day passes but Sir Charles makes a pun or picks a poc——, that is to say, asks a riddle. The total number perpetrated must be by this time very large, but some of the more celebrated ones have been remembered for 30 years. When travelling to Melbourne by steamer in 1877, the steward came round in the afternoon and asked Sir Charles if he would take tea. "Oh yes!" was the reply, "without T I would be odd." On this or another voyage he asked "When are the passengers and a ship of the same mind? When the passengers are telling stories and the ship is lying to." But he has not allowed these recreations to diminish an enormous output of daily work, and he will carry with him into retirement the admiration and goodwill of the world. The Royal Observatory at Greenwich, where he started work in 1841, before the transit-circle or even the altazimuth was "born or thought of," will note with pride the honourable termination to his long and successful career.

The recreations of a distinguished astronomer have sometimes important scientific consequences. Prof. Simon Newcomb is fond of playing games of "Patience," and one variety of the numerous possible forms suggested a mathematical enquiry. This ultimately found its way to Major P. A. MacMahon, F.R.S., who recognized in it a problem allied to one already considered by him some 15 years ago (Phil. Trans. 1893), which he calls the "compositions of multipartite numbers by a graphical method." Returning to this field of work, he has produced a first-rate memoir of some 70 pages of the 'Philosophical Transactions,' read on December 6, 1906. If it is permitted in the same breath to refer to his approaching marriage (fixed for February 9, the day following the R.A.S. Annual Meeting), we can particularize among general and hearty congratulations that on the completion of this vast memoir which might otherwise have divided his attention at an important period.

No name has yet apparently been assigned to the minor planet TG, which is of special interest in two ways. It furnishes, with Jupiter and the Sun, an example of three bodies in stable equilibrium at the corners of an equilateral triangle, and its orbit is so large that it wanders beyond the orbit of Jupiter. Is it too frivolous a suggestion that the name should remain TG? Readers of Rudyard Kipling will be familiar with this combination of letters, though they may not be able to interpret them. Indeed I made several unsuccessful enquiries in India before getting the true reading. The meaning is obviously (from the context in Kipling's story) a "globe-trotter," and I thought at first that TG might be a *simple inversion* for GT; but when the real answer was obtained it was, of course, simple enough—a TG is a "travelling gentleman," not an unsuitable designation for the asteroid that wanders farthest afield.

'In the Days of the Comet,' by H. G. Wells (MacMillan, 1906: price 6*s.*) needs no recommendation to those who know Mr. Wells's books. The Earth passes through the tail of the comet, and though "machinery went on working," all living things were overwhelmed by a green vapour which "masked and stilled everyone." They revived, however, and found a wonderful change: all their characters had become quite beautiful. The astronomical references are made with Mr. Wells's usual care and skill. We are first introduced to the comet when it was being studied as a mere speck by a solicitor's clerk named Parload, who

> had commandeered an old opera-glass from his uncle and . . . . with infinite labour and the help of a very precise article in *The Heavens*, a little magazine that catered for those who were under this obsession, he had at last got his opera glass upon the new visitor to our system from outer space. He gazed in a sort of rapture upon that quivering little smudge of light among the shining pin-points—and gazed. My troubles had to wait for him. (p. 15.)

(Incidentally we wonder whether this august magazine appears in this passage under a pseudonym?) The comet grows until it becomes "a great white presence, brighter than Jupiter, and casting a shadow on its own account" (p. 43); grows still until it "looked brighter than the Moon because it was smaller, but the shadow it cast, though clearer cut, was much fainter than the Moon's shadow" (p. 68); yet further until "it was the most splendid thing this sky of Earth has ever held" (p. 84); till there was

> a curious accentuation of the common alternation of night and day. . . . One waited for its rising, and yet each night it came as a surprise. Always it rose brighter than one had dared to think, always larger and with some wonderful change in its outline; and now with a strange, less luminous greener disc upon it, that grew with its growth, the umbra of the Earth. It shone also with its own light, so that this shadow was not hard or black, but it shone phosphorescently and with a diminishing intensity where the stimulus of the Sun's rays was withdrawn. . . . . It changed the starless sky about it to an extraordinary deep blue, the profoundest colour in the world, such as I have never seen before or since. . . . (pp. 147–8.)

Very few, if any, of our imaginative writers have taken the trouble to write like this about scientific facts and possibilities; even where we do not catch the point at once there is food for thought. Mr. Wells deserves our thanks for a careful and interesting piece of work.

From a bookseller's catalogue of second-hand *astronomical* books before 1800:—

> Mallet (A. M.). Les Travaux de Mars, ou l'Art de la Guerre, divisé en trois parties: La première, enseigne la Méthode de fortifier toutes Sortes de places Réguliers et Irréguliers: &c., &c.

Alongside the above we may record the fact that our respected contemporary *Nature* deliberately prints the title Aërography (two dots over the *e* and all) on p. 231 of a recent issue, as the proper heading for references to Mars. A month or two back a similar slip was noted as a joke, but it looks as though the epidemic were becoming serious.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. MARCH, 1907. No. 381.

ANNUAL GENERAL MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1907 February 8.

Mr. W. H. MAW, *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

MR. SAUNDER read the Minutes of the last Annual General Meeting, which were confirmed.

Mr. Newbegin presented the Report of the Auditors of the Treasurer's accounts for the year 1906. They had examined the accounts, and found and certified the same to be correct. The cash balance in hand on 1906 December 31 was £19 4*s*. 5*d*., as compared with £320 12*s*. 4*d*. on 1905 December 31. The overdraft to the bankers of £400 which appeared in last year's account had been paid, Stock having been sold to the amount required. The Auditors mentioned that the excess of expenditure over income appeared to be due mainly to the large printing bill, and they were glad to note that the Society's publications were now being printed by another firm. The Auditors had inspected the books, instruments, and other effects in the possession of the Society, which appeared for the most part to be in good condition, but they were sorry to note that no action had been taken with regard to the suggestion made by the Auditors last year as to disposing of certain unusable instruments lying in the cellars.

Prof. Turner. With reference to the Auditors' remarks about the printing bill, I should like to say that the chief reason for its large amount is entirely due to the excess of matter printed. Fellows will remember that last summer a large volume of Memoirs was printed. Without that comment the remarks of the Auditors might seem to have a sinister meaning.

The Secretaries then presented the Report of the Council to the Society. Mr. Lewis read the names of Fellows and Associates who had died during the year 1906, and extracts from the obituary notices which had been prepared, the complete list being as follows:—*Fellows*: Raphael L. Bischoffsheim, Rev. John Bone, E. H. R. Coleman, T. R. Dallmeyer, R. I. Finnemore, J. H. Freeman, Joseph Gledhill, C. J. Joly, John Joynson, A. E. Nicolls, Robert Rawson, W. J. Reynolds, P. E. Sewell, William Simms, and the Rev. George Venables; *Associates*: Samuel Pierpont Langley, J. A. C. Oudemans; and an *Hon. Member*, Miss Agnes Clerke, who died early in this year.

Mr. Saunder read extracts from the Proceedings of Observatories, which included, besides the Royal and University Observatories, reports from the following amateur astronomers:—Rev. T. E. Espin (Wolsingham Observatory), S. A. Saunder (Crowthorne), W. E. Wilson (Daramona Observatory, Westmeath), Sir Wilfrid Peek (Rousdon, Mr. C. Grover, Observer in charge), Mr. W. H. Maw, Mr. Franklin-Adams, Sir Wm. Huggins, Mr. Alex. Roberts (Lovedale, S. Africa), and Mr. John Tebbutt (Windsor, N. S. Wales).

Mr. Lewis read the list of subjects reported on in the Progress of Astronomy, 1906, with extracts from some of them:—Minor Planets, Meteoric Astronomy, Solar Activity, Solar Prominences, Double Stars, Variable Stars, Small Satellites, Stellar Spectroscopy, Star Catalogues, Comets, Geodesy, the Astrographic Chart and Catalogue, and Universal Time.

The President. The Council have awarded the Society's Gold Medal to Prof. Ernest W. Brown, F.R.S., of Haverford College, Pennsylvania, U.S.A., for his researches in the Lunar Theory.

Mr. Maw then, in a complete but concise address, gave a brief history of the Lunar Theory, and specially of the research in which Prof. Brown is now engaged. It appears that he is the seventh astronomer to whom the Gold Medal of the Royal Astronomical Society has been awarded for work in connection with the lunar theory, his predecessors being Baron Damoiseau (1831), M. Jean Plana (1840), Prof. P. Hansen (1860), Prof. J. C. Adams (1866), M. Delaunay (1870), and Dr. G. W. Hill (1887). Distinction was drawn between some and others of the ten or eleven Lunar Theories now in existence, which can be first broadly divided into two classes, literal and numerical, and can be further divided into two classes, in one of which the first assumption is the Keplerian ellipse, the other class having the variational curve as a first approximation. It is to the latter of these classes that Prof. Brown's theory belongs, and it therefore comes under the heading of periodic solutions. At the close of his address Mr. Maw said:—I much regret that Prof. Brown is not with us this evening to receive the Medal personally, but a combination of circumstances—amongst them the serious illness of a relative—has rendered it impossible for him to cross the Atlantic at the present time. This being so, I have been in communication with Sir Edward Grey, the Secretary of State for Foreign Affairs, and I am glad to say that he has kindly arranged for the Medal to be forwarded in the Foreign Office bag to His Majesty's Chargé d'Affaires at Washington, by whom it will in due course be transmitted to Prof. Brown.

I will now ask Mr. Lewis to receive the Medal on Prof. Brown's behalf, and to transmit with it our most sincere hope that he may long be spared in full health and strength to carry out the important researches to which he has devoted himself.

(The Medal was handed to Mr. Lewis, and the President continued:—)

Before passing to other business there is another matter connected with our Medallist on which I should like to say a few words. I have not alluded to it in my address because it has nothing to do with the award of the Medal, but it will, I think, be of interest to the Fellows generally. As many present are aware, Prof. Brown is an Englishman who has been long resident in America, and who has for the past sixteen years been connected with Haverford College. That association, however, will be broken in the ensuing summer, and next autumn Prof. Brown proceeds to Yale University. It is exceedingly gratifying to know that his work on the Lunar Theory, which he has been able to carry on at Haverford under most favourable conditions, will not be interrupted by this change. By a letter received from Prof. Brown I learn that not only have the Yale authorities recognized the importance of his work by arranging special facilities for its continuance, but they have also most generously undertaken to provide the funds required for both the preparation and the publication of the Lunar Tables, which will form the natural outcome of our Medallist's labours.

Sir David Gill. I rise to propose that the Report of the Council be received and adopted, and that it be printed and circulated in the usual manner, together with the President's Address and the Report of the Auditors. I do not know whether it is usual to make any remarks in support of this proposal, for it is a good many years since I last attended the Annual General Meeting of the Society, but I am sure that all present will join me in congratulating the President on the clear and able manner in which he has set forth the reasons for the award of the Medal, which I feel sure will convey to our friends in America an assurance of the interest that we feel in their astronomical work.

The proposal was seconded by *Prof. H. H. Turner*, and carried unanimously.

The President appointed Mr. T. E. Heath, Mr. H. P. Hollis, and Mr. R. Inwards as Scrutineers of the Ballot for the election of Officers and Council for the ensuing year. The Ballot was then taken, and the following were declared duly elected:—

President: H. F. Newall. *Vice-Presidents*: Sir W. H. M.

Christie (Astronomer Royal), Sir David Gill, Major P. A. MacMahon, W. H. Maw. *Treasurer*: Major E. H. Hills, C.M.G. *Secretaries*: Thomas Lewis, S. A. Saunder. *Foreign Secretary*: Sir William Huggins. *Council*: Sir R. S. Ball, Bryan Cookson, P. H. Cowell, A. C. D. Crommelin, Prof. F. W. Dyson (Astronomer Royal for Scotland), Alfred Fowler, J. W. L. Glaisher, J. A. Hardcastle, A. R. Hinks, E. B. Knobel, E. J. Spitta, Prof. H. H. Turner.

Mr. Newall then took the Chair amid acclamation.

Mr. Goodacre proposed and *Mr. Bryant* seconded:—"That the thanks of the Society be given to the President, Vice-President, and Members of Council who now retire for their services in promoting the objects of the Society."

The motion was put to the Meeting by the President (Mr. Newall) and carried unanimously.

Mr. Criswick proposed and *Mr. Rolston* seconded a resolution that the thanks of the Meeting be given to the Auditors of the Treasurer's accounts and to the Scrutineers of the Ballot.

The following gentlemen were elected Fellows of the Society:—

Edgar T. Adams, 3 Warkworth Street, Cambridge.

Robert Jonckheere, Observatoire Stella, Roubaix, France.

John Stewart, Chief Officer, R.M.S. 'Empress of China,' The Willows, Wallasey, Birkenhead.

Samuel Veevers, Normanton, Kimberley Drive, Great Crosby, near Liverpool.

The following Candidates were proposed for election as Fellows of the Society:—

Arthur Neville Brown, M.A., Ludgrove, New Barnet, Herts (proposed by Col. E. E. Markwick).

Henry Cooper, 19 Cromer Road, Eastville, Bristol (proposed by W. F. Denning).

Phanindralal Gangooly, M.A., Premchand Roychand Student, University, Calcutta, India (proposed by Asutosh Mukhopadhyay).

William Newsam McClean, 42 Durdham Park, Bristol (proposed by Sir David Gill).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

SION COLLEGE, VICTORIA EMBANKMENT.

Wednesday, 1907 January 30.

F. W. LEVANDER, *President*, in the Chair.

Secretaries: J. G. PETRIE.
J. A. HARDCASTLE.

MR. PETRIE read the Minutes of the previous Meeting, which were confirmed and signed.

The names of nine Candidates for Membership were read and passed for suspension, and the election by the Council of three new Members was unanimously confirmed.

The President said that, before the commencement of the ordinary business, he desired to call attention to the very great loss that astronomy had suffered during the last few days by the death of Miss Agnes Mary Clerke. He then briefly alluded to Miss Clerke's principal contributions to astronomical literature, and read the following resolution which had been passed by the Council at their Meeting that afternoon:—"That this Council, having learned with deep regret of the death of Miss Agnes Mary Clerke, a greatly esteemed and distinguished Member of the Association from its inception, and for six years a Member of the Council, desire to put on record their profound sorrow at the loss which astronomy has sustained, and to express their condolence and deep sympathy with her bereaved family; and that a copy of this resolution be sent to her brother, Mr. A. St. J. Clerke. 1907 January 30."

This resolution was unanimously accepted, and passed by the Meeting as a conjoint one.

The President said there were two further points he wished to mention. Mr. Crommelin had kindly consented to give a lecture on "Halley's Comet" at the February Meeting of the Association. Members were doubtless aware that Mr. Crommelin had been paying a great deal of attention to the perturbations of Halley's Comet. Then he had just received a letter from Prof. Boccardi, the Director of the Observatory at Turin, stating that a society, to be called the Società Astronomica Italiana, had been formed. It was based on the lines of the British Astronomical Association, with a subscription of 10 lire per annum, and would publish a Journal, partly in Italian and partly in French. The new society wished to be put on their list of exchanges, to which the Council had very readily assented. He might also mention that some few months ago *Popular Astronomy*, in taking notice of one of the numbers of the British Astronomical Association's 'Journal,' expressed regret that there was nothing in the United States at all resembling this Association, and the writer wished very much that there were.

Mr. Crommelin read a paper on "The Total Solar Eclipse of 1241 October 6." He said that we possessed fuller and more graphic details of the physical appearance of this eclipse than might be expected considering its early date. Mr. Cowell had examined the matter in connection with his new value for the rate of acceleration of the Moon's node relatively to the Sun, and he found that the eclipse could not possibly have been total at *all* the places at which it was noted that stars appeared, for these formed a belt considerably wider than the known width of the shadow. It was necessary to assume that stars were visible with about 20″ of the Sun's diameter uneclipsed; it thus became of interest to examine how the planets Jupiter, Venus, and Mercury were placed. Jupiter was below the horizon, but both Venus and Mercury would doubtless be visible, as might also Spica, Arcturus, Vega, and Regulus.

Mr. Maunder thought that the fact that Venus was low in the sky at the time of the eclipse would rather tend to draw attention to the planet, and the fact that Arcturus was favourably situated would render it probable that it was one of the "stars" seen. It was the first star to be noticed after actual sundown, so he did not think there was any difficulty in accepting that Mercury, Venus, and Arcturus might have been seen very readily with 20 seconds or even a good deal more of the Sun still unobscured.

Mr. Lynn said that one of the most remarkable things about the account of the eclipse was not so much what it did record as what it did not. That a total eclipse could have been seen and no appearance of the corona referred to was surely extraordinary.

Mr. G. F. Chambers said he would like to point out that there still lay buried among the old antiquarian records of this country, and also of Europe, a large number of astronomical records which deserved to be dug up and published. He believed if the work were taken in hand by some Member with a little spare time it would bring to light a very large number of records of considerable antiquarian and scientific interest.

The President, in thanking Mr. Crommelin for his paper, said it would be a very good thing if some Member would take the hint thrown out by Mr. Chambers.

Mr. Maunder gave a *résumé* of a paper by *Capt. Daunt, D.S.O.*, on "Prominences, 1906," in the course of which he remarked that the fact that April was the most active month for prominences, and July and November were the least active months, was somewhat striking, because July was for sun-spots about the most active month of the year. Then, whereas the zones of greatest activity in the matter of prominences in the N. hemisphere were +15° to +30°, and in the S. hemisphere −20° to −30°, sun-spots had now reached a mean latitude of about 13° in either hemisphere. The numbers given in the paper showed that

Capt. Daunt had been able to accomplish a most important amount of much needed work.

Mr. Newbegin was very glad to see somebody else was trying to do work of this sort on similar lines to those he had adopted. He found that prominences occurred more or less within the sun-spot zone. It was quite true, as Mr. Maunder had said, that it did happen that, when the Sun was very busy with spots, they would perhaps find very few prominences, and *vice versa.*

The President then gave a *résumé* of another paper by *Capt. Daunt*, giving a description and illustrations of a home-made wooden equatorial stand.

Mr. Goodacre read a paper on "Some Ancient Lunar Formations," illustrated by lantern-slides. He explained that one of the objects he had in view was to bring before the Members who used small telescopes a means by which they might do some interesting work upon the Meon. Those who possessed only such telescopes often thought that by reason of the smallness of their instruments they were unable to do any useful or interesting work in connection with the Moon; but if they would take up the study of some of the ancient ring-formations they would find a most useful branch of selenographical work open before them.

Mr. Saunder asked whether Mr. Goodacre could give observers any criterion to enable them to distinguish between new and old formations. He thought from one part of his paper that Mr. Goodacre did as many others had done, namely, classed the bigger as old and the smaller as new.

Mr. Goodacre, in reply, said it was entirely beyond the power of anyone to lay down anything like dogmatic lines on which one could say which formation was older than another.

Mr. Hardcastle exhibited some lunar slides, and called attention to a paper on the subject read by M. Puiseux in November before the French Astronomical Society. • Mr. Hardcastle agreed as to the difficulty and danger of elaborate theories as to what had happened on the Moon.

Mr. Lynn read a paper on "The Variations of Light in η Argûs." He pointed out that the extraordinary nature of these changes rendered the star in question a sort of connecting-link between temporary and variable stars.

The President said that the literature of η Argûs was rather extensive, and he was sure that they were all gratified at having heard a paper on the subject from Mr. Lynn.

The Rev. T. E. R. Phillips read a paper by the *Rev. R. Killip* on "The Disappearance of Jupiter's 4th Satellite."

Mr. Burns exhibited a chart showing the proper motions of 2500 stars taken from Bossert's Catalogue." Each of the stars shown had a P.M. of at least 10″ per century, and more than half of them had a P.M. exceeding 20″ per century.

The President said there were two or three other papers, one by *Col. Markwick* on "The Occultation of ν Capricorni," and one by

Mr. Samuel Stuart, who was very fond of the archæological side of astronomy, on "The Hindu Kaliyuga Epoch." These would appear in the next issue of the Journal.

Mr. Hardcastle contributed the first part of a paper on "Tides on the other Side." He said one of his reasons for bringing forward the paper was to elicit the opinion of the Association on a point about which he wanted to have, if not a quarrel, at any rate a discussion with some educational authorities. In a recent University Higher Local Examination the question was asked, "What is the cause of the tides?" and he was asked by a student "What was the right answer?" Now he wanted to know what answer the examiner expected, and he wished to fortify himself by laying the matter before the Association and hearing to what extent his difficulties and arguments appeared relevant. Mr. Hardcastle then proceeded to read the first part of his paper dealing with the unsatisfactory and misleading explanations of the text-books on the subject.

Mr. Crommelin said that he quite sympathized with the writer in the line he had taken, that the ordinary text-books did far more harm that good in their attempts to explain the tides. The true explanation of the tides was practically the same as that of the solar perturbations of the Moon, and it was not to be supposed that anybody who could not understand the one could follow the other. Certainly it was not fair to expect candidates for the Higher Local Examinations to understand the true action of the Moon in producing the tides.

Mr. Seabroke said he also sympathized with Mr. Hardcastle in criticizing the explanations commonly given. There had been more rubbish written about the tides than anything else.

The President said that he had seen the second part of Mr. Hardcastle's paper, in which he would point out what he considered the best way an explanation could be given of the cause of the tides.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday evening, February 20, at the Institution of Civil Engineers, Great George Street, Westminster, *Dr. H. R. Mill*, President, in the Chair.

Mr. Edward Mawley presented his Report on the Phenological Observations made during 1906 by observers in various parts of the British Isles. The most noteworthy features of the weather of the phenological year ending November 1906, as affecting vegetation, were the dry period lasting from the beginning of June until the end of September, and the great heat and dryness of the air during the last few days in August and the first few days in September. Wild plants came into flower in advance of their usual dates until about the middle of April, after which time they were, as a rule, to about the same extent late. Such early spring immigrants as the swallow, cuckoo, and nightingale reached these islands somewhat behind their average dates. The only deficient farm-crop, taking the country as a whole, was that of hay, all the others being more or less over average. The yield of apples was about average in all but the north of England and in Scotland, where there was a very scanty crop. Pears and plums were everywhere very deficient, whereas all the small fruits yielded moderately well. As regards the farm-crops, the past year proved even a more bountiful one than that of 1905.

Mr. Richard Inwards read a paper on "The Metric System in Meteorology," in which he called attention to the advisability of adopting some uniform system by all the meteorological observers upon the globe.

On the Connection between Disturbed Areas of the Solar Surface and the Solar Corona [*].

THAT there exists a general connection between the state of disturbance of the Sun's surface and immediate surroundings, and the solar corona, is well established. There is a great similarity in type in the photographs of the solar corona of the years 1870, 1882, 1893, and 1905, all of them years of maximum solar activity, marked by many and great outbursts of sun-spots and prominences. A quite different type of corona corresponds to the years of minimum solar activity, as witness the photographs of 1878, 1889, and 1901; while yet again a type of corona intermediate between those of years of maximum and minimum solar activity is associated with the years of waning sun-spots and prominences, 1886 and 1896. But it is open to question whether the sun-spots or the prominences are the more intimately connected with the streamers which constitute the characteristic forms of these variations in coronal type. In the Stonyhurst photographs of the solar corona of 1905 August 30, taken at Vinaroz, Spain, the streamers seem in general to mark the regions of the prominences rather than of the sun-spots. The longest streamers photographed, consisting of two long wings with an intermediate shorter streamer, extend from latitude $-40°$ to $-90°$ S., outside the limits of the spot zones, while another streamer is placed near the Sun's north pole. Moreover, the general tendency of all the more marked streamers is poleward, while the fine group of prominences in the north-east quadrant is manifestly associated with three fine streamers. In the lower corona the complicated structures of arches and vortex rings are attached to the prominences. In this connection it will be interesting to quote from Mr. Evershed's account of the prominences of 1905 * :—"The polar regions of the Sun have again been nearly quiescent, although the high latitude zones of activity have made a distinct advance toward the poles. These prominences were found to be most frequent between the parallels 65° and 70° in both hemispheres, and the limits of prominence-formation at the close of the year may be put at +75° and −75°." It would seem that the poleward tendency noticed in the streamers of the corona, in the main, was also characteristic of the prominences observed during the year, which again were most frequent in latitudes greater than those of the spot-zones.

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[*] This article, taken from the *Astrophysical Journal*, 1906 December, may be taken as a summary of Father Cortie's deductions from his eclipse observations of August 1905, the details of which are given in full in the 'Transactions of the Royal Irish Academy,' vol. xxiii. Section A, part 1. The paper was read before Section A at the last Meeting of the British Association at York. In the present article the diagram and tables which support the argument are omitted.

In the photographs of the corona of 1905 August 30, the sun-spot zones are marked by some straight radial bright rays in the south-west quadrant, and by some beautiful plumes in the south-east quadrant. These rays and plumes are well shown in the third plate of the Stonyhurst series, exposed for 15 seconds near the beginning of totality on a Schleussner's "Observatory" plate. The instrument employed was the full aperture of the 4-inch lens of the 20-foot coronograph belonging to the Royal Irish Academy. In the discussion of the results of the eclipse, presented to the Royal Irish Academy, it was shown that the area on the solar surface to which the projected images of these plumes converged was situated in the south-east quadrant, the mean latitude of the position being approximately −21° S. Four sun-spot groups were visible on the Sun on the day of the eclipse, and the largest, whose area was one-thousandth of the visible disk, had a mean latitude of −20° S., and was removed about 41° from the Sun's east limb. Moreover, of the two limbs, the east limb on the day of eclipse had the greater number of sun-spot centres of greater activity during the year in its vicinity, including among them, though on the invisible hemisphere of the Sun and removed some 51° from the limb in latitude −16°, that centre which had been the seat of the outburst of the great sun-spot group of February 1905, the greatest observed for thirty years. If the plumes converged to an area behind the visible disk, then it was possible that they were connected with the great February spot; if to an area on the visible hemisphere, the possibility was that they were attached to the spot seen on the day of the eclipse, especially as it was near the place of a large group visible to the naked eye during the preceding July. Since writing the memoir for the 'Transactions of the Royal Irish Academy,' the life-histories of the spots and faculæ that occupied these two regions of disturbance have been carefully studied from the Stonyhurst sun-spot drawings.

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* 'Monthly Notices,' lxvi. p. 227, February 1906.

It appears that the region of the Sun in longitude about 320°, latitude 19° S. in January to 30° S. in July, was occupied by a spot of varying size in each rotation from January to June, and in the last-named rotation, and in the July rotation, by bright faculæ. After this date the region became quiescent and remained so during the eclipse. Also the region in latitude about 16° S. and longitude from 53° to 70° was occupied by a large group of spots in each of the five rotations from May to the end of August.

This region was disturbed, according to the Stonyhurst drawings, from May 11 and was active on August 30, the day of the eclipse, and afterward. Therefore it follows that the connection, if any, between the coronal plumes and a disturbed area on the Sun must be postulated of this second area. Is the connection merely one of coincidence of position, and not a real convergence of the plumes to the spot area? The probability of the reality of the connection seems greatly enhanced by the study of a similar region in 1893, latitude 8° S., mean longitude about 45°, which was the area of convergence of a like system of rays or plumes in the lower corona of April 16.

This area, which was continuously active from a month before the eclipse to nearly a month afterward, and again broke out into disturbance in September and November, was placed on the eclipse day about 34° from the Sun's west limb with position-angle 237°. In Prof. Schaeberle's Skeleton Diagram of Corona, plate ix., facing p. 89 in his discussion of the eclipse photographs in the Report of the Total Solar Eclipse of April 16, 1893, is an object marked π, of which we read (p. 100, *loc. cit.*):—"The heaviest of a number of much inclined and conspicuous streamers issuing between P.A. 230° and P.A. 250° of the Sun's outline . . . . . on account of the numerous crossing of streamers from greater position-angles the individual directions cannot be ascertained with certainty. At about P.A. 210° the bunching up from intensities is so great that from height $=10'$ to height $=15'$ an apparent increase of density in the corona takes place." In his discussion of results Prof. Schaeberle distinguishes coronal arches and coronal streamers; but, so far as can be made out from a comparative study of the eclipse photographs of Prof. Schaeberle and those of the English observers, Mr. Kearney and Mr. Taylor, the structure described in the above quotation is very like that named as plumes on the Stonyhurst plates of 1905.

Without subscribing entirely to the mechanical theory of the solar corona as advocated by Prof. Schaeberle, and above all to the statement (p. 126, *loc. cit.*) that "there is no evidence to indicate that either electricity or magnetism has anything to do with the arrangement of coronal matter in the Sun's neighbourhood"; and, moreover, admitting the sorting effect on the lighter materials ejected from the Sun due to the pressure of light, yet, in any discussion of the constitution of the corona, the great part played by the ejective forces in the Sun, actively displayed in sun-spots and prominences, must be fully admitted, and any theory which rejects or overlooks the effect of such forces must be regarded as incomplete. The present paper gives highly probable evidence that not only in general is a characteristic type of solar corona associated with sun-spot and prominence activity, but that definite structures in the corona are associated with definite areas of activity of sun-spots and faculæ. A. L. C.

Stonyhurst Observatory,
1906 July 27.

The Sun-spots of 1906 *November.*

It has been found necessary, at all events for the winter months, when the Greenwich series of photographs of the Sun is necessarily much interrupted, to wait for fully two complete months before attempting to number the spot-groups, as a shorter interval does not give time for all the necessary supplemental photographs to arrive from India and to receive proper examination. The report, therefore, on the sun-spots of 1906 November could not be got ready in time for the February issue of the *Observatory.*

With the appearance on October 29, at the east limb of the Sun, of the fine stream Group 6001, the remarkable secondary minimum of last October came to an end and a period of revival set in. The solar activity, so far as shown by the sun-spots, has continued to increase right up to the present time, February 15; but during November last the revival was only commencing. The stream, Group 6001, passed off at the west limb on November 10, two days after another stream, or rather procession of groups, No. 6003, 6004, and 6006, had come into view at the east limb. A feeble echo of this disturbance was seen in the same longitude, but in the northern hemisphere, in Groups 6009, 6010, 6011, and 6012. Three days before Group 6006 had disappeared at the west limb, the fine double spot, Group 6015, came into view at the east limb, and as it reached the central meridian the chief disturbance of the month, an irregular procession of six groups, began to be visible at the east limb. This procession consisted of three parts: Group 6026, a stream in high latitude; Groups 6021 and 6023, together making up a stream in mid-latitude; and Groups 6024, 6027, and 6030, together making up another high-latitude stream. The last three appear to represent the return, or rather the revival, of Group 6001, the stream with which the month commenced.

Rotation 711 began 1906 November $18^{d}\cdot33$.

Notes on the Principal Spot-Groups.

Group 6004.—A large, well-defined, composite spot. Area from 400 to 500.

Spot-Groups in 1906 November.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	d	°	°	
6002	Oct. 30	Nov. 2	4	Oct. 27·8	284	+ 5	A few small spots.
3	Nov. 7	14	8	Nov. 13·3	66	−10	A small spot.
4	8	20	13	13·7	61	−10	Large composite spot.
5	8	9	2	13·8	59	+14	Small faint spot.
6	8	20	13	14·4	51	−12	Large cluster *f.* Gr. 6004.
7	10	16	7	10·2	107	−10	A few small spots.
8	10	20	11	15·8	33	+ 8	A small spot.
9	11	15	5	13·1	69	+16	One or two small spots.
6010	12	18	7	13·6	62	+12	Two spots.
1	13	14	2	12·9	71	+ 9	A very small spot.
2	13	14	2	14·1	55	+11	A very small spot.
3	14	15	2	16·0	30	−19	A pair of small spots.
4	16	18	3	18·2	2	+ 8	One or two small spots.
5	17	28	12	23·5	291	−24	Fine double spot.
6	18	20	3	14·3	53	−15	Small spot *s.* Gr. 6006.
7	19	20	2	24·0	286	+11	Small faint spots.
8	19	23	5	24·5	279	−27	Small spots *sf.* Gr. 6015.
9	22	28	7	21·8	315	+15	Small stream.
6020	22	23	2	23·3	294	+ 9	One or two small spots.
1	23	Dec. 5	13	29·7	210	+11	Fine double spot.
2	25	Nov. 25	1	23·8	288	−20	Small spot *nf.* Gr. 6015.
3	25	29	5	30·3	202	+ 9	Stream *f.* Gr. 6021.
4	25	Dec. 6	12	30·7	197	+18	Fine regular spot.
5	26	Nov. 28	3	21·8	314	− 9	One or two small spots.
6	26	Dec. 4	9	28·7	223	+22	Irregular stream.
7	26	3	8	Dec. 2·1	179	+18	Small spot *f.* Gr. 6024.
8	27	Nov. 28	2	Nov. 22·9	300	−11	Small spot *np.* Gr. 6015.
9	27	28	2	28·9	220	+ 1	Small faint spot.
6030	28	Dec. 1	4	Dec. 1·5	187	+16	Small stream *sf.* Gr. 6024.

Group 6006.—A number of spots irregularly arranged, but principally collected in a single cluster, *f.* Group 6004. Area from 250 to 300.

Group 6015.—A fine well-defined spot with double nucleus, eventually breaking up into two regular spots. Area about 120.

Group 6021.—A fine double spot, eventually breaking up into two regular spots. Area about 250.

Group 6024.—A fine regular spot. Area about 200.

Group 6026.—One or two small spots, developing later into a long irregular stream. Area on November 29, about 180.

E. WALTER MAUNDER.

The Planets and Planetary Observation.

[Continued from p. 96.]

PROF. BARNARD in 1895 wrote:—"Venus has been examined on a number of occasions in the 36-inch [at the Lick Observatory] when the planet was beautifully defined. Surface-markings were nearly always present, but they were very illusive and at no time could a satisfactory drawing be secured. The visible markings were large dusky spots essentially like those described on Mercury but much fainter. Nothing was seen of the singular system of narrow dark lines shown in recent years by some observers to cover the surface of this planet. Every effort was made to show them by the reduction of aperture and the use of solar screens and various magnifying-powers. They were also looked for with the 4-inch finder. Previous attempts with the 12-inch here also failed. I am confident the faint elusive spots seen with the great telescope were real, but whether they were of a permanent nature it was impossible to tell, for the same spots were not recognised with certainty at different observations. The impression, however, was that they were not permanent." (*Ast. Nach.* 3760.)

Mr. P. Lowell after a careful survey of Venus said:—"The markings are long and narrow, but, unlike the finer markings on Mars, they have the appearance of being natural not artificial. They are permanently visible whenever our own atmospheric conditions are not so poor as to obliterate all detail on the disc. The markings, which are a straw-coloured grey, bear the look of being ground or rock, and it is presumable from this that we see simply barren rock and sand weathered by aeons of exposure to the Sun. They are perfectly distinct and conclusive as to the planet's period of rotation being performed in the same time as her orbital revolution. I have seen the markings when their contours had the look of a steel engraving. They are invariably visible and have been seen by everyone at the observatory who has made any systematic observations of the planets. The drawings of all the observers agree as well as, if not better than, do their drawings of Mars. The markings were so distinct as to preclude the possibility of illusion." (*Monthly Notices*, lvii. pp. 148 & 402.)

Mr. H. MacEwen sums up his observations in 1900 as follows:—"The gaseous envelope surrounding Venus is between 400 and 500 miles deep. The solid surface of Venus is seldom if ever seen. The planet has a quick rotation, *i. e.* similar to the Earth's rotation period. The exact or even approximate duration of this rotation period is very difficult to ascertain."

M. C. Flammarion remarks:—"The immense atmosphere of Venus certainly prevents any detail on its surface from being perceived. The grey spots now and again seen upon the planet are the effects of contrast due to solar illumination, while the less definite shadings are of an atmospheric nature incapable of furnishing us with any serious data as to the rotation of the planet. Everyone sees after his own fashion and draws after his own fashion. No one has ever seen on the surface of Venus any clearly characteristic spot analogous to those shown on the discs of Mars and the Moon. The maps of Venus made up to the present time are illusions." (*Knowledge*, 1897 November.)

These sweeping assertions will not be generally accepted, but they certainly have some foundation, if they are not fully warranted by the facts. When we reflect, however, upon the large number of able and eminent astronomers who have seen and drawn the spots it is impossible to conceive that they could one and all have fallen ready victims to illusion. The phases of Venus mirrored in our instruments form bright and beautiful pictures, but is it possible we must regard them as underlying a deception and a snare capable of affecting the equanimity and imaginations of all those who pay their telescopic devotions at the shrine of the fair goddess?

In the autumn of the winter of 1901 Venus appeared entirely free from visible markings, according to Mr. MacEwen, Major Molesworth, and Mr. Bolton. Mr. Smith, of Dalbeathe, obtained photographs of Venus in 1902 January, and five of these (January 13) clearly show the south cusp of Venus more rounded than the north one, and also the interesting feature that the north cusp is shown pointed on two of these prints, those taken at $5^h\ 30^m$ and $5^h\ 35^m$, whereas at other times on the same evening it was rounded.

In 1895 the Rev. T. H. Foulkes from a number of observations concluded that it was impossible to suggest what Venus did in the way of rotation.

At Malta, however, Mr. Foulkes says he was strongly of opinion that the period must be short, ± 24 hours.

In 1899 August, the Abbe Th. Moreaux agreed with Antoniadi, in *M. N.* lviii. p. 317, that the scattering of light by atmospheric particles of the planet hides all surface-markings from our view. The symmetrical shadings seen on the dichotomized disc are very likely mere contrast effects. Under such circumstances we are not authorized to pronounce ourselves in favour of any rotation period.

In 1902 Herr Arendt at Berlin observed the planet and concluded that the visible disc is composed of masses of dense clouds concealing the real surface. From the apparent motion of certain grey spots parallel to the terminator he was led doubtfully to the idea of a rotation occupying about 24 hours in length. (*A. N.* 3803.)

At Bristol since 1881 the writer has made occasional observations of Venus, but the markings have never presented the same distinctness as they did in the year mentioned. Exceedingly faint spots have been glimpsed at times, but they appeared mere differences of tone in the atmospheric envelopes of Venus and could not be traced with certainty from day to day. In 1903, May 19, $8^h 30^m$, the south horn seemed decidedly more rounded than the northern and not so bright. The limb bright all round, with a faint shading inside concentric with it, which is quite a normal aspect. On May 21, 8^h, a large indefinite faint dusky spot near centre of illuminated disc. In the morning sky of 1903, October and December, Venus was also carefully examined, but there were no definite irregularities visible (10-in. reflec. p. 312).

The Rev. T. E. R. Phillips, as the result of many observations, says (1906):—"I have never been able to trace any *certain* motion in the markings on the planet's surface. These markings appear to me to be of three kinds chiefly, viz.:—

"(i.) Those which are dependent on the planet's phase, such as a dusky streak concentric with the limb commonly seen when the planet is dichotomized or in the crescent stage.

"(ii.) Faint irregular patches and occasional bright spots or streaks, which exhibit little consistency or order in their appearance as observed at considerable intervals of time.

"(iii.) Bright regions sometimes seen at the cusps like polar caps. I have not sufficient data to justify me in forming any opinion as to the planet's rotation-period. At one time I thought the way in which some of the markings apparently preserved their positions unchanged with regard to the terminator indicated a long rotation-period, but if these are phase effects no rotation value can be based upon them. Moreover, I am inclined to think that the other irregular markings are largely clouds and atmospheric phenomena, and that through the dense atmosphere the real surface of the planet is rarely if ever seen at all."

Mr. A. Stanley Williams, of Hove, writes (October 1906):—"I made a somewhat exclusive series of observations of the planet over 20 years ago and am certain that I have seen *change* in a short period of time. For instance, on one occasion there was an almost black spot visible near the terminator. More exactly it resembled a blackish spot seen through mist. About half an hour later when looked for again this marking could no longer be perceived or traced as a definite object.

"The large, faint, indeterminate light and dark markings certainly varied from night to night and even on the same night, but it would be necessary to make assumptions as to identity in order to derive a rotation-period from these features.

"On several nights of good seeing a dark shading seemed to advance on the disc in the course of the same night, and this to recur at nearly the same time on two consecutive nights; but this effect *might* be only apparent and dependent on the change from daylight to twilight and darkness.

"The only marking at all resembling a canal was a dark streak near the south limb, but as the south part of the disc was shaded whilst the limb was very brilliant, this was probably only an effect of contrast.

"At present the spectroscopic results are as diametrically opposite as the visual ones, so that it seems to me the question of the true rotation-period of the planet must remain doubtful until either the spectroscopic method attains to considerably greater refinement or a small definite spot can be followed long enough to show certain motion and its rate. Desultory observations hardly seem likely to be successful, but only the most severe systematic research."

Apart from the observations quoted, markings on Venus have also been observed by Brenner, Elger, Fontsère, Lyman, Löschardt, Langdon, Lepper, Molesworth, Niesten, Phillips, Terby, Wilson, Worthington, and many others at different times.

We shall make no attempt to discuss the various observations briefly quoted. Assuredly there are enough of them for the purposes of a thorough investigation, but whether or not it would be possible to reconcile a fair number and extract a satisfactory result as regards both the visible surface-markings and rotation-period of Venus may well be open to question. Certainly any attempt to combine and harmonize the materials in a comprehensive manner must entail enormous labour and prove an exceedingly difficult undertaking.

Prof. Schiaparelli searchingly reviewed the work up to 1890, and perhaps no one could have done it better, but his conclusions are not favourably received on all hands and they are not affirmed by a considerable portion of the evidence. The task of assorting the accumulated data would have been much easier and possibly a reliable deduction could have been made from them had some of the contributors endeavoured more earnestly to steer clear of the shallows of imaginative observation.

Flammarion's summary of past results is very valuable. It includes charts of the Cytherian markings by Bianchini, Niesten, Brenner, Lowell, and others, as well as numerous drawings. And of all the results thus collected together, perhaps Mr. Lowell's are the most curious. At Arizona, Venus seems to have shown her spots in palpably obvious characters; they appear to have exhibited sharply bounded outlines and to have formed a great spoke-like figure, in strange contrast to what has come under the eyes of other observers. To provide an explanation of anomalies of this sort is impossible. The observers have doubtless all done their best, and if we are confronted with startling contradictions we must put them down to "personal equation" and to the difficulties appertaining to the research.

We have said that it will be impossible for us to give a complete *résumé* of observations in recent years. This has been partly rendered unnecessary by the publication of several memoirs in which such results have been suitably collected and discussed. Among these are the following:—

G. V. SCHIAPARELLI. Considerazioni sul moto rotatorio del pianeta Venere. Milan, 1890.

F. TERBY. Bull. de l'Acad. Royale Belgique (10 figures), 2nd **series, xlix.** Brussels, 1880.

——. Ditto (54 figures), 3rd **series, xx.** Brussels, 1890.

L. NIESTEN. Ditto (14 figures), 3rd series, xxi. Brussels, 1891.

E. L. TROUVELOT. Observations of Venus and Mercury. 88 pp. (100 figures). Paris, 1892.

CAMILLE FLAMMARION. General Discussion of Observations of Venus. 32 pp. (32 figures). Paris, 1897.

F. LÖSCHARDT. Die neuesten Hypotheusen über die Rotation des Planenten Venus. Wien, 1891.

Mr. P. Lowell claims that the spectroscopic observations made by Mr. Slipher support the period of 225 days. These results he confirmed by taking photographs of the spectrum of Mars under similar conditions, which, on being measured, gave a period of $25^{h}\ 35^{m}$, the true period being $24^{h}\ 37^{m}$. He points out that a rotation-period for Venus of 24^{h} would give a velocity about 90 times that found on the plates measured. Some other efforts have been made to ascertain the rotation-period of Venus by spectroscopic observations. In 1901 March–May, Prof. Belopolsky employed the 30-in. Pulkowa O.G. to this end, and the results favoured a short rotation-period (*A. N.* 3641). The work of two other eminent spectroscopists confirms this, but the method cannot be regarded as conclusive, as there are great difficulties in the way of obtaining very exact results.

Mr. Lowell's conclusion is therefore distinctly contrary to Prof. Belopolsky's and some others, and the spectroscopic method seems likely to be as full of contradictions as the telescopic method.

The following is a summary of the rotation-periods found for the planet at various times :—

Rotation of Venus.

1.	1666	23^{d}	0^{h}	0^{m}	J. D. Cassini.	Bologna.
	1666–7	23^{h}	20^{m}	55^{s}	,,	
2...	1726–7	24^{d}	7^{h}	54^{m}	F. Bianchini.	Rome.
3...	1732–40	23^{h}	15^{m}	0^{s}	James J. Cassini.	Paris.
4...	,,	23	21	30	,,	,,
5...	1788	23	28		J. H. Schröter.	Lilienthal.
6...	1789	23	21	7·98	,,	,,
7...	1789–81	23	20	59·04	,,	,,
8...	1796	24^{d}			Flaugergues.	Viviers.
9...		24^{h}	15^{m}	43^{s}	J. J. Lalande.	
10...	1801	23	22		Fritsch.	
11...	1833	23	24		J. H. Mädler.	
12...	1841–2	23	21	21·9345	F. de Vico.	Rome.
13...	1890	224^{d}	16^{h}	49^{m}	G. V. Schiaparelli.	Milan.
14...	1891	23^{h}	0^{m}	0^{s}	L. Niesten.	Brussels.
15.	1892	23	49	28	E. L. Trouvelot.	Cambridge, U.S.A.;
	,,	24	5		,,	Meudon, France.
16...	1892–6	23	57	36·37728	L. Brenner.	Istrien.
17...	1896	23	57	36·2396	,,	,,

1.—From observations of bright spots near the terminator. Cassini considered the period somewhat doubtful.

3.—From a re-discussion of his father's observations.

Transits of Venus.—Somewhat similar phenomena appear to have been seen on Venus during transits as have been remarked on Mercury when projected on the Sun. When the planet is only in part on the solar disc a bright fringe has been traced round that part of the limb outside the periphery. And even when Venus has been completely on the Sun a luminous annulus or aureola has been sometimes said to surround her, though there have been many contradictions.

To exemplify this we may quote a few observations made on the occasion of the last transit in 1882 December 9.

Rev. S. H. Saxby said a luminous margin to the dark limb was very apparent with a breadth quite appreciable, perhaps 2″ or 3″. By an optical illusion a grey patch of light appeared on the planet's disc, its extreme diameter being ⅔ that of the planet. 3¾ O.G.; p. 55.

P. F. Denza.—No aureola. Planet's disc not quite black; it had a tint between a light red and dark yellow.

A. S. Williams.—Light fringe suspected, but there were no luminous specks on the planet. At ingress the outside limb of Venus showed a light fringe caused by the presence of clouds on the planet. 2¾-in. O.G.; p. 74.

J. L. Stothert.—No sign whatever of luminous margin or of white spot on disc. 6-in. O.G.; p. 60 to 150.

R. Grant, Glasgow.—No atmospheric effects around planet. 5½-in. O.G.; p. 120.

C. L. Prince.—A halo of yellowish light surrounded her disc, not a ring, but a diffused light, now here, now there. 6¾-in. O.G.; p. 100.

S. P. Langley.—When the planet was half on the disc there was a bright marginal segment along about 30°, greatest width ¼ planet's radius. 6-in. O.G.; p. 244.

4.—James Cassini concluded that Bianchini's period really included 25 rotations.

5.—This seems to have been his first determination.

6.—Observations of dusky spots.

7.—Observations of bright objects at the horns.

8.—The period was derived from a bright spot seen only on 4 days, 1796 July 7–13.

9.—Lalande gives this period, but on what authority?

11.—This period agreed with several of the spot-motions, but it cannot be regarded as a definitive value.

12.—From a comparison of about 10,000 measurements. The rotation-period derived varied between $23^h\ 53^m$ and $23^h\ 11^m$.

13.—From an exhaustive discussion of past observations.

15.—These periods conformed with certain spots, but Trouvelot says the value was about 24 hours.

16.—Observations by Stuyvaert, 1892 Nov. 4, and Villiger, 1896 May 31. 1552 rotations. The period is given to the ten-thousandth part of a second, but it may just possibly be 223 days in error!

17.—From his own observations.

Keeler at same place also saw it. 2¼-in.; p. 70.

Rev. S J. Johnson.—No trace of any ring of light. 2¼-in. O.G.; p. 150.

Rev. F. Howlett.—Disc of Venus a very dark Indian-ink brown, no indications of an atmosphere or central spot of light. 3-in.; p. 80.

M. Horner.—There was a distinct prismatic fringe round the edge of the planet when about three-quarters on disc. 5-in. O.G.; p. 85.

F. Brodie.—"When the planet's disc was halfway on the Sun I saw with great clearness the twilight caused by the atmosphere of Venus all round that portion of her disc off the Sun. It was a soft white light, assuming a ruddy hue next to the edge of the planet's disc." 8½-in. O.G.; p. 80.

It would serve no useful purpose to multiply such quotations. They would contain many contradictions. During ingress, and when the planet's disc had only partly entered on the Sun, a light border was certainly visible around the dark limb outside the Sun's periphery. This seems well corroborated by much reliable testimony. When Venus is near inferior conjunction, her dark unilluminated part has sometimes been distinguished. The twilight at the margin of the limbs apparently shows up the dark disc as the effect of contrast.

In leaving the inferior planets Mercury and Venus we do so with a consciousness that very little has been certainly revealed as to their surface configuration. Indeed, we know practically nothing about it. After all the earnest application of telescopic observers during three centuries, the problem of their real aspect and axial velocity remains unsolved. The difficulties have proved insuperable. Telescopes are powerful, eyes are good, and there are nights of transparent loveliness, but we cannot probe the atmospheric veils which hide the real features of the two planets. Successive generations of observers in the future will, however, continue to study these objects, and circumstances may ultimately arise to favour their efforts.

Both Mercury and Venus are attractive to the eye, Mercury for the rareness of his glittering visits, and Venus for her dazzling beauty. If they are apt to disappoint telescopic students, from the difficult circumstances affecting the work, the phases have always furnished an interesting spectacle ever since Galileo first saw Venus emulating the form of the Moon about three centuries ago.

[To be continued.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Ancient Eclipses.

Gentlemen,—

The letter of Mr. Lynn in your issue of January, and a recent one of Mr. Cowell in the columns of *Nature*, strongly confirm the views which I have already expressed that in very few (if any) cases have we a sufficient historical account of an early eclipse to justify us in using it as a basis for scientific deductions of any kind.

In the eclipse with which Mr. Lynn deals, the historical record seems pretty clear. The battle of Stiklestad took place on Wednesday, July 29, A.D. 1030, and during the battle it became dark. The historian does not state that this was an eclipse or give such a description that nothing but an eclipse could have satisfied it. But as there was (according to our tables, which, to that extent, I am not disposed to dispute) an annular eclipse of the Sun on the 31st of August, A.D. 1030 (which was not Wednesday), it is proposed to displace the historic account of the battle by 33 days in order to explain the alleged darkness. Can anyone rely on this displacing process as infallibly correct? A mistake as to the day on which Olaf was defeated and slain (but his family were restored to power in a few years) is, I think, very improbable. But if the historian may have erred by more than a month as to the date of the battle, may he not have similarly erred as to the alleged occurrence of darkness during it? Olaf was the great promoter of Christianity in Norway, and was afterwards regarded as a saint and martyr. What if the darkness at mid-day on the day of his death was a legendary addition to the facts, borrowed from a similar occurrence during the Crucifixion? If the historian is to be trusted for one purpose, why not trust him for all purposes? And if he is untrustworthy, why not throw overboard his testimony altogether?

The other case to which I refer is the so-called eclipse of Agathocles, the account of which comes from Diodorus Siculus, who wrote more than 250 years afterwards, and does not seem to be regarded as a very reliable historian. How far his narrative absolutely excludes any eclipse except that dealt with by Mr. Cowell I am unable to say. I should wish to see the point discussed. But when Diodorus states that Agathocles saw the eclipse on the morning after he left Syracuse, the possibility of making a mistake of some days is sufficiently obvious. If he saw it at any time on his voyage from Syracuse to the coast of Africa it might easily be set down to the day after he started, after the lapse of several years even if recorded by Agathocles himself. Nor, probably, would astronomers be in the least shaken in their conclusion as regards the eclipse if an old manuscript of Diodorus turned up in which for "the day after" was substituted "the fifth day after." They would merely conclude that Agathocles started four days earlier than they supposed—both dates being equally consistent with the historic narrative. But the change of four days would make a difference as to the *place* where the eclipse was seen, and this is the point with which Mr. Cowell is dealing. Agathocles had to avoid the Carthaginian fleet in escaping, and he took the opportunity of breaking out when the Carthaginians had gone to intercept some provision-ships that were trying to enter the harbour of Syracuse. But did he escape towards the north or towards the south? Had he a fair wind or a contrary wind? Did he start in the morning or in the evening? The answers to all these questions would affect his distance and direction from Syracuse at the time that the eclipse was seen by him. The historic narrative is silent on all these points, and Mr. Cowell proceeds to supply the hiatus by conjecture. Such conjectures may be plausible or otherwise, but they never can afford a basis for science. Agathocles (if, as Mr. Cowell supposes, his vessels were slower than those of the Carthaginians) would probably prefer to slip out at nightfall, so that the darkness might soon screen him from his pursuers; but as he took the opportunity of starting when they had gone out to capture the provision-ships, he had to take his time from these ships instead of selecting it for himself. As the provision-ships were coming from the south, it might be supposed that the Carthaginians moved to the south to meet them, so that Agathocles would naturally escape to the north: but what if the provision-ships adopted a circuitous course, thinking it would be easier to get in from the north, as the Carthaginians would be on the lookout in the opposite direction? Supposing that he had the same start, Agathocles's chance of escape would probably be better in calm weather than in a fresh breeze; but as his movements were evidently determined by those of the provision-ships, no stress can be laid on this consideration. The whole procedure seems to me mere guesswork.

I trust that a serious effort will be made in the case of any early eclipses that are to be used as a basis for scientific conclusions to distinguish, 1st, what we know of the eclipse on purely historic grounds, with any assistance from tables, cycles, &c.; and 2nd, taking the eclipses given by our present tables, is it clear that there is only one which fulfils the historic conditions? To which may be added, 3rd, is it clear that nothing but an eclipse can satisfy the conditions of the historic narrative?

With regard to Ptolemy's 'Almagest,' I would suggest the question, Did Ptolemy use cycles or any other methods of computing these eclipses, or did he find the dates which he records clearly stated in historic narratives? Ptolemy must have been in possession of much more accurate records than any that have come down to us if he fixed these dates by means of them only. As regards many of the eclipses in the 'Almagest,' I suspect that he was in pretty much the same position as Mr. Lynn with relation to the eclipse of St. Olaf. Three of the eclipses Ptolemy may have observed himself, but all the others occurred long before his time, and it is not likely that he possessed any such full and accurate records as enabled him to fix their dates without the use of cycles; in which case another astronomer, with the same *data*, might arrive at a different result, as has occurred in such a marked manner with the eclipse of Thales—which, I think, was probably identical with the eclipse of Larissa.

I remain,
Truly yours,
Dublin, 1907, Jan. 25. W. H. S. MONCK.

THE basis upon which my conclusions from ancient eclipses rests is the coincidence between the natural interpretation of the records and the results of computation from my formulæ. In addition to several lunar eclipses, there are now eleven solar eclipses which support my formulæ: −1062, −762, −648, −602, −584, −430, −309, +197, +1030, +1239, +1241. I admit that for various reasons the eclipses of −648, −309, +1239, and +1241 are not delicate tests between other formulæ and mine. I admit also the possibility (*à priori*) that Diodorus may have written "one" day when the truth was "five" days, and that the Norwegian chronicler, who is certainly 33 days wrong in the day of the eclipse, may also have been inaccurate as to the place. But my contention is that my formulæ and the records form a self-consistent system that confirm each other. My formulæ are right; Diodorus was right in saying one day; the Norwegian chronicler was right as to the place of the eclipse; again I am correct in inferring that the narrative of Thucydides refers to Athens, &c., &c.

Now this is the crucial question, *Can these coincidences be accidental?* The contention against me is that I have made a series of erroneous assumptions as the basis for my calculations. Let me grant, for the sake of argument, that I have done so: what, then, would be the antecedent probability of one set of formulæ suiting them all? Xenophon talks of a great cloud: suppose I am wrong in assuming this to mean an eclipse, what is the chance of finding an eclipse within a definite period of 25 years and total at a definite place? These are astronomical questions, or arithmetical questions, which cannot be answered without taking some trouble. Let me put it in another way. I first began to express confidence in my results on finding agreement between the eclipses of −762, −648, −430, −309, and +197. Subsequently I found the eclipse of −1062 also in agreement. I then published. Now suppose my results were wrong, is it conceivable that the lunar eclipses and the remaining five solar eclipses should fall into line?

There is a further difficulty in the view that an eclipse can easily be found to fit a wrong hypothesis. This is not the case, but let us grant that the chance is as great as the chance of a wet day. Then the reasonable expectation would be that sometimes the chance did not come off, and that no eclipse could be found; that sometimes it would come off doubly, and that there would be two eclipses which would do equally well. If, on the other hand, total eclipses at particular places are rare occurrences (as surely we all know they are), the chance of an eclipse when the record really refers to something meteorological, or the chance of two eclipses when only one eclipse is recorded, is negligible.

I will take this opportunity of explaining my change of attitude with regard to the eclipses of Larissa and Thales. In my first paper, based on five eclipses in addition to the eclipse of Agathocles, I stated that I rejected them as not based upon contemporary evidence. I knew that these eclipses had been used to deduce a wrong conclusion, and I did not know at that time that the date of Larissa was impossible. I therefore assumed that eclipses that gave an erroneous result must have been inaccurately described; but my confidence in the other six eclipses was such that I felt that my conclusions could stand the strain of the disagreement, as I expected, of these two eclipses. It was Mr. Nevill's paper that first disclosed the fact that these eclipses fitted in with my formulæ. The error was not Herodotus's or Xenophon's. The error lay in the tacit assumption that the astronomical theory of the time was complete, and that no unknown causes remained to be allowed for; in other words, that from eclipses the constants of a preconceived theory were to be looked for, instead of the measurements of pure geometry. This same difference of conception marks not only my treatment of eclipses, but my discussion of modern observations as well. Theory should be brought in as late as possible; its function in the present instance is to indicate that a change in the length of the year is more probable than an unforeseen motion of the node.

I have lately had brought to my notice the remarkable manner in which the date now assigned to the eclipse of Larissa fits in with what we previously knew of chronology. In −606 the Chaldæans took Nineveh, in −604 they defeated the Egyptians at Carchemish; for the next three years they are lost sight of, and in −600 or −599 they invade Judah. The capture of Larissa exactly fits into this gap of three years in the current Assyriological knowledge. P. H. Cowell.

William John Burchell and η Argûs.

Gentlemen,—

Burchell was not an astronomer, his principal scientific attention was devoted to botany. But it is impossible to read his account of his travels in South Africa without perceiving that he was quite competent to make astronomical observations, and did thus determine the positions of many of the places which he visited. These extended from 1811 to 1815; and about ten years later he started on another voyage to South America, in which continent he travelled from 1825 to 1829. To him we owe the filling in of a gap in the history of that extraordinary variable star η Argûs, the changes in which from 1810 to 1902 are laid down in a curve in the late Miss Clerke's 'System of the Stars' (2nd edition, p. 108). Prof. Newcomb, in his book 'The Stars, a Study of the Universe,' spells Burchell's name erroneously, as Birchell (p. 125), and thinks that he somewhat over-estimated the star's magnitude in 1827. The following year he considered it to be between the first and second magnitudes, and Sir John Herschel found it about the same when he first observed it at the Cape in 1834, and so it remained until the great outburst towards the end of 1837, which came to a climax a few weeks afterwards, when the star's brightness equalled that of α Centauri, and then slightly diminished, only to be succeeded by a greater gradual rapid outburst, though still reckoned of the first magnitude when Sir John Herschel last observed it on April 14th, 1838. This was shortly followed by the most remarkable of its outbursts, which in 1843 brought it very nearly equal to Sirius in brightness. From this time it declined gradually, and ceased to be visible to the naked eye in 1868.

Burchell's observations were communicated in a letter to Manuel Johnson at Oxford, which is given in Sir John Herschel's 'Results of Observations at the Cape of Good Hope,' p. 35, and is dated 9th June, 1845. He states that he had repeatedly noticed it when in South Africa and always found it of about the fourth magnitude—the same at which Halley estimated it at the first recorded observation obtained by him at St. Helena in 1677. But at São Paulo in Brazil, on the 1st of February, 1827, he noticed that it was then (as already mentioned) of the first magnitude; and at Goyaz, in the same country, on the 20th of February, 1828 (when he measured its distance from β Crucis), he found it "of the second magnitude at least."

Burchell, as I have remarked, was evidently a careful and painstaking observer. Dr. Swainson says of him that "he must be regarded as one of the most learned and accomplished travellers of any age or country." It is to be regretted that he never published any account of his later journeys, and that even that on South Africa (which appeared in two quarto volumes in 1822) deals only with the explorations in 1811 and 1812, and a third projected volume never reached publication. He brought back from South America a large collection of plants, which, with his manuscripts, are now at Kew Gardens. His death occurred at Fulham on the 23rd of March, 1863, in the eightieth year of his age.

And now a few words respecting the star itself and the constellation in which it is placed. It will be noticed that that great writer whose recent decease has been such a loss to astronomy (Miss Clerke) speaks of the star, in her 'System of the Stars,' under the designation η Carinæ, and indeed it is quite at the bottom of the ship, or rather beyond it. (The word "carina" well represents the Greek τρόπις applied to that part of Argo by Ptolemy, but it really means what we should call the hull, the usual word for which in Latin is "alveus.") But that is not the principle of nomenclature in this abnormal constellation, the lowest part of which is not visible in Northern Europe. Prof. Newcomb well remarks ('The Stars, a Study of the Universe,' p. 125) that "Bayer's maps of this constellation are so erroneous that little correspondence can be found between his figures and the heavens." The real distributor of the stars in the southern hemisphere was Lacaille, who made an effort (which Bayer did not, as many have erroneously supposed) to make the sequence of his letters in some measure correspond with that of the gradations of brightness in the stars. Hence, in the case of Argo, he retained, amongst Bayer's letters, only α for that of Canopus, undoubtedly the brightest. Bayer has an η, but that is not the variable, nor (as Prof. Newcomb remarks in the page referred to above) is there any reason to identify either of the seven stars which he marks as "*d*" in the saxum, or rock beyond the ship, with it. Lacaille's β, γ, δ, ζ Argûs are of the 1·7, 1·9, 2·0, 2·3 magnitudes respectively; and though ε is now 1·7, it is not unlikely it may have been fainter when observed by Lacaille. All these stars are in places very different from those assigned in Bayer's map of the constellation. But what I am principally anxious to point out is that in recognizing divisions of the ship into Carina, Puppis, Malus, and Vela, from the parts named by Ptolemy, the brightest stars (all those, indeed, having a Greek letter) are attributed to the whole ship, and only the fainter ones to its subdivisions. Halley's so-called "Robur Carolinum" has only faint stars, and seems to have been intended to supplant part of the old "saxum."

Yours faithfully,

Blackheath, 1907, Feb. 4. W. T. Lynn.

PUBLICATIONS.

A new Durchmusterung*.—The precise difference between a star-catalogue and a Durchmusterung has probably not yet been authoritatively stated, but a Durchmusterung is usually taken as defining a publication in which *all* stars down to a certain magnitude, usually a comparatively small one, are included. Herren Ambronn's 'Sternverzeichnis' may be considered, therefore, as coming under this definition, as it contains all stars from North to

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* 'Sternverzeichnis enthaltend alle Sterne bis zur 6·5 Grösse für das Jahr 1900·0.' Von J. und R. Ambronn. Berlin: Julius Springer, 1907.

South Pole, although they are all as bright or brighter than magnitude 6·5. There are 7796 stars of this magnitude in the sky whose places have been collected and reduced from the catalogues of the Astronomische Gesellschaft, the Madras Catalogue, the Argentine General, the Washington, Greenwich, Radcliffe, and other Catalogues, to epoch 1900·0, and given to a degree of accuracy defined by tenths of a second of R.A. and whole seconds of arc in declination. The statistical table in the Introduction, showing the distribution in R.A. and in magnitude, is interesting. The magnitudes of the stars and their annual variations of both elements are given, also the catalogue from which the place is derived and the reference to the B.D., so that it forms altogether a very useful reference-book for occasions when only approximate places are needed. The appendix gives the proper motions of a very large proportion of these stars, and a list of 43 binary stars with elements of their orbits as deduced by Mr. Aitken.

The Report of the Variable-Star Section of the British Astronomical Association, edited by Col. Markwick, has been recently issued. This volume of the Memoirs was rather beyond the financial resources of the Association, and has been printed by the help of funds supplied partly by the Government Grant of the Royal Society and partly by the generosity of two Members of the Association. Its excellence has quite justified this munificence.

The Publishers, Messrs. King, Sell, & Odling (*Knowledge* Office), have kindly sent us a copy of the 'Science Year-Book' for 1907. It is a platitude that no one keeps a diary after the first month of the year; and if this is so the compilers of this book have rather overdone things, for there is not only a page to be filled for each day of the year, but many pages at the end of the book for an elaborate systematizing of correspondence, accounts, memoranda, &c. The information, general and scientific, at the beginning of the book, seems especially well chosen, the astronomy being very complete. The meteorological information in the heading for each day is a special feature of this publication.

In nearly the same category as the above is Mr. Arthur Mee's card calendar called 'The Heavens at a Glance,' of which the present is the eleventh annual publication. It contains a calendar of astronomical facts for the year, and charts of parts of the heavens. It can be bought from Mr. Mee, Llanishen, Cardiff, for sevenpence, post-free.

Messrs. Zeiss have sent us a rather different sort of astronomical almanac, by Mr. R. H. Bow, of Edinburgh. This is a card diagram, showing the paths of the planets among the stars throughout the year, with other information of similar kind ingeniously shown.

NOTES.

Comet Notes.—*Astr. Nach.* 4154 contains new elements of Comet 1905 IV. by Dr. E. Weiss. He has used Dr. Wolf's photograph of 1905 Jan. 14 and three normal places from observations during March, April, May, 1906; he obtains the following parabola, which represents the places with no residual exceeding 5″:—

T	1905 Oct. 18·035 Berlin M.T.	
ω	158° 39′ 56″	1907·0.
☊	342 18 3	
i	4 16 10	
log *q*	0·52356	
q	3·339	

The comet is now at distance 5 from the Earth, and its brightness ¼ of that in Jan. 1905. As there is a chance of its being reobserved, Dr. Weiss gives the following ephemeris for Berlin midnight:—

	R.A.	S. Dec.
	h m s	° ′
1907 Mar. 1	15 0 59	21 10
17	14 59 38	21 19
Apr. 2	14 55 21	21 16

Prof. Barnard gives in *Astr. Nach.* 4154 an account of the trail of an unknown faint comet, which has been found on a plate exposed at Mount Wilson. 1905 July 25^{d} 18^{h} 55^{m} G.M.T.: position 18^{h} 23^{m} $41^{s}·2$, S. 20° 31′·9; daily motion +229^{s}·5, S. 17′·9. He publishes this in case the comet may prove to be periodic, and in this event the position could be utilised. A. C. D. C.

Minor Planet Notes.—*Astr. Nach.* 4155 contains a rediscussion of the orbit of 588 TG by Dr. Bidschof, using all the observations of last year. His elements differ considerably from Dr. Berberich's in the eccentricity and position of the perihelion, but the values for node, inclination, and mean motion are sensibly the same. An advance copy of his ephemeris was sent to Dr. Wolf, at Heidelberg, and by its aid the planet was recovered on Jan. 22. The observed R.A. is 11^{h} 53^{m} 2^{s}, which is 46^{s} less than that of Bidschof's ephemeris, 121^{s} greater than that of Berberich. The observed Dec. is −6° 44′ 11″·5, Bidschof's being −6° 50′, and Berberich's −6° 29′. It will be seen that the observed place is intermediate between the other two, but much nearer Bidschof's. The planet is now of the 15th mag., and a very difficult object at such a low altitude.

It is very fortunate that it has been detected, as for several years it will be in very low South Dec., and out of reach of European observers. It is to be hoped that some of the southern observatories will step into the gap, but in any case the recovery of the planet on its return north is probably now assured.

Epoch, 1906 Feb. 22·5 Berlin M.T.

	Bidschof.	Berberich.	
M	43° 45′ 37″	48° 57′ 24″	
ω	129 24 11	120 25 50	1906·0.
☊	315 31 7	315 34 7	
i	10 16 36	10 20 53	
φ	8 10 15	9 38 43	
μ	294″·70	295″·13	
log a	0·72041	0·71999	

Ephemeris for Berlin Midnight, by Dr. Bidschof.

	R.A. h m s	Dec. ° ′
1907 Mar. 3	11 40 12	−6 40·7
11	11 36 8	−6 24·8
19	11 31 58	−6 5·9
27	11 27 57	−5 45·2

A new planet was detected on Greenwich plates exposed on 1903 Aug. 6, 31, Sept. 1. Dr. Kreutz finds that it is not identical with any known planet, and designates it LX^{a}. He gives the following circular elements in *Astr. Nach.* 4154:—

Epoch	1903 Sept. 1·5 Berlin M.T.
u	38° 58′
☊	287 19
i	7 21
μ	709″·9
log a	0·4659

This is the first planet discovered at Greenwich.

The following new planets have been discovered at Heidelberg and Taunton, Mass., U.S.A.:—

WU	1906 Dec. 7	Metcalf.	XC	1906 Dec. 21	Kopff.
WV	7	”	XD	23	”
WW	20	Kopff.	XE	18	”
WX	21	”	XF	18	Metcalf.
WY	22	Lohnert.	XG	18	”
WZ	22	”	XH	18	”
XA	21	Kopff.	XJ	Jan. 22	Lohnert.
XB	21	”	XL	22	”

WQ is identical with 167 Urda, XK with 129 Antigone.

475 Ocllo was photographed by Dr. Metcalf, 1907 Jan. 11^{d} 15^{h} 30^{m}·5 G.M.T., in R.A. 7^{h} 36^{m} 9^{s}·2, N. Dec. 48° 16′·0.

A. C. D. C.

Obituary.—H. Chamberlaine Russell. The death is announced of Mr. Russell, formerly Government Astronomer of New South Wales and Director of the Sydney Observatory. Mr. Russell, who had previously been Assistant in the Observatory under the Directorship of Mr. Smalley, on the death of that gentleman in August 1870 was appointed to succeed him and held the offices above mentioned until the end of the year 1904, when he had been connected with the Observatory forty-six years in all. He was elected a Fellow of the Royal Astronomical Society on February 10, 1871, and contributed several papers to the Society, generally on observations of a miscellaneous nature made at the Southern Observatory. He took part in the Australian expedition to observe the total solar eclipse of December 1871, but the weather was unfortunate. His observations of the Transit of Venus, 1874, appear to have been especially successful, judging from the weight assigned to them in the final discussion. He was elected a Fellow of the Royal Society in 1886.

Mira Ceti.—The maximum of this star was predicted for December 20. In a letter to *Nature*, dated December 1, Mr. Backhouse wrote:—"It may be well to note that this variable star is exceptionally bright at its present maximum, the magnitude being about 2·0. As usual when the star is bright, it is less red than the average." In *Ast. Nach.* 4141, Prof. Hartwig notes that on December 4 and 7 it was as bright as α Arietis. At its maximum in 1904 the star appears to have been of magnitude about 3·0, in 1900 about 3·4, and in 1890 about 4·0. It may be remembered that in 1779 Sir W. Herschel recorded Mira as little inferior to Aldebaran, and in 1839 it was even brighter, and the maximum which has just passed will class with these. In *Knowledge* for February, Mr. P. M. Ryves, giving an account of his observations, says that the most rapid rise was from the 6th to the 3rd magnitude between October 26 and November 14, 1906, and that it remained as a second magnitude star for three weeks.

The Royal Observatory and the L. C. C.—The Report of the Committee appointed to enquire into the working of the electric generating station at Greenwich is referred to on another page of this number. Supplementing the paragraph there quoted it may be said that the chief points of the Report are:—

1. There is clear evidence that the working of the engines does give rise to vibrations of the ground which are felt at the Observatory.
2. This is shown by the nadir observation. The reflected image of the wire in the transit-instrument moves in such a manner as to show that the mercury is undergoing forced oscillations whose period agrees with that of the impulses due to the inertia of the reciprocating masses in the station engines.
3. It is possible, by using a very thin film of mercury on an amalgamated metal surface of slight curvature, to damp these vibrations so that the observation is possible. The film is still fluid enough to assume the form of a horizontal plane under the action of gravity, but apparently shares the motion of the ground with the instrument.
4. Though the vibrations may thus not be perceived, they are still there, and this may have an injurious effect, but of this there is as yet no proof.
5. Up to the present there is no evidence of any material effect on the observation of stars. Conclusive evidence on this point will take a long time to accumulate. It would be premature to say that the vibrations do not interfere with such work as the micrometer observation of double stars.
6. If evidence presents itself later that the vibration of the gronnd is prejudicing the work of the Observatory, the question should be re-opened.

The Printing Chronograph.—It is not known whether this type of apparatus has as yet come over to England, but from notes by MM. Boquet and Chatelu in the *Bulletin Astronomique* it is learned that a chronograph which prints on the sheet the time when the tap registering a transit is made is being perfected, and is being used at several French observatories—Besançon, Toulouse, Nice, and at Paris. We believe that the scheme, or perhaps one of the schemes, for a printing chronograph originated with M. l'Abbé Verschaffel, of the d'Abbadia Observatory; the one in use at Paris was made by M. P. Gautier, and from the last note published it is understood that several details have been improved, that it now prints hundredths of seconds, and that the results are excellent.

Dr. Isaac Roberts's Photographs. — Mrs. Dorothea Isaac Roberts, who writes to *Ast. Nach.* 4154 from Chateau Rosa Bonheur, By-Thomery, S.-et-M., says that she is making a catalogue of the 2485 original negatives of stars, star-clusters, and nebulæ taken by her late husband, that a limited number of copies will be at the disposal of astronomers specially interested in photographic astronomy who apply to her, and that positives on glass reproduced from the negatives will be lent for micrometric measurement. A preliminary catalogue is published in the above-mentioned number of the *Ast. Nach.*

The Rotation of Jupiter.—Mr. Stanley Williams has published his observations during the Opposition of 1905–6 of the Red Spot on Jupiter in *Ast. Nach.* 4150. The rotation-period derived from his observations of 1904–5 was $9^{h}\ 55^{m}\ 41^{s}.57$, whilst that from the series now published is $9^{h}\ 55^{m}\ 41^{s}.46$. Referring to a statement by Mr. T. E. R. Phillips in this magazine, that the rotation-period of the Red Spot Hollow when derived from one Opposition to another was shorter by $0^{s}.4$ than that derived from the observations at each opposition, Mr. Williams points out that, comparing his observations of the Red Spot at the first Opposition with those at the second, the period comes out $9^{h}\ 55^{m}\ 40^{s}.84$, which shows a difference in the same direction as that noted by Mr. Phillips, but larger. Mr. Williams thinks that his result from the observations of 1905–6 is worthy of some confidence, because it is confirmed by observations of the Red Spot Hollow made by a method suggested by Mr. Maw which is likely to render the result free from systematic error.

A Magnetic Storm and Aurora Borealis.—On the afternoon of Saturday, February 9, a magnetic storm was recorded at Greenwich and at the Physical Laboratory, Kew, larger than any that has happened since 1903 October 31. The storm began at Greenwich at 2.15 P.M. on Saturday, and lasted till 3 o'clock on Sunday morning. In a letter to *Nature* Dr. Chree says that at Kew the storm began and ended practically at these same times, and that the largest movement in declination occurred between $8^{h}\ 15^{m}$ P.M. and $8^{h}\ 45^{m}$ P.M., in which interval the needle moved

57′ to the west, and then 73′ to the east, which is also corroborated by the Greenwich sheet.

In the evening of Saturday a brilliant display of aurora was seen over a great part of the United Kingdom, described by several persons in Lincolnshire and the Midlands as a display of coloured streamers shooting rapidly from the northern horizon to the zenith. Mr. G. A. Clark, the observer at the Aberdeen University Observatory, notes that the display lasted from 6 P.M. until after midnight.

There happened to be an unusually large number of spots on the Sun at the time. Four very large spots were visible to the naked eye, an almost unprecedented event.

LARGE SUN-SPOTS.—The largest of the spots, or rather groups, mentioned above, which was on the central meridian on February 10, was of immense size, 18° in length by 8° in breadth in all, though not very compact. In addition to these there has since been another large group during the month, about 10° long by 5° broad, on the central meridian on February 22. There have already been nine spots on the Sun visible to the naked eye this year.

AN ASTRONOMICAL HOAX.—A paragraph has appeared in several papers of the past month stating, on the authority of Prof. Matteucci, of the Observatory of Mount Vesuvius, that a collision between the Earth and a comet would take place during March, with the awful consequences that are usually supposed would attend such an occurrence. The statement appears to have originated in an Italian newspaper, and to this the Professor wrote, entirely denying that he made such a suggestion.

A CORRECTION.—We regret to have to note an error in the obituary notice of Mr. William Simms on p. 108 of our last number. Mr. W. Simms was cousin to Mr. James Simms, the present head of the firm, and not the brother, as stated. Mr. William Simms, Senr., who was taken into partnership by Edward Troughton in 1826, had two brothers, Frederic Walter, a surveyor and engineer, who, among other things, superintended the construction of a large part of the main line of the South-Eastern Railway, and James Simms, a manufacturer of ships' compasses in Hatton Garden. Mr. W. Simms, who has recently died, was the son of this James Simms, and the present Mr. James Simms is the son of William Simms, Senior.

MR. A. S. EDDINGTON, of Trinity College, Cambridge, and Chief Assistant at the Royal Observatory, Greenwich, has been awarded the Smith's Prize at Cambridge for his essay on "The Systematic Motions of Stars," read before the Astronomical Society in November last. His fellow prize-winners are Messrs. Blanco-White, Nicholson, and Page.

THE Isaac Newton Studentship at Cambridge has been awarded to Mr. H. R. Hassé, of St. John's College, who will carry on a course of research in physical optics.

The Adams Prize at Cambridge for 1907 has been awarded to Prof. E. W. Brown for his essay on "The Inequalities in the Moon's Motion due to the Direct Action of the Planets."

The next Meeting of the Royal Astronomical Society will be on Friday, March 8; of the British Astronomical Association on Wednesday, March 27.

From an Oxford Note-Book.

The Report of the Committee, consisting of Lord Rosse, F.R.S., Sir Benjamin Baker, F.R.S., and Mr. J. A. Ewing, F.R.S., appointed to enquire into the working of the London County Council generating station at Greenwich in its relation to the Royal Observatory, is now made public. We shall all be grateful for the following paragraph; and it is to be remembered that one member of the Committee of three was appointed to represent the interests of the London County Council, so that his opinion is at least impartial:—

> In our judgment the station should not have been placed on the site at all. The matter has gone so far that to remove the station now or to stop the completion of the second portion would involve a much greater expense than the circumstances warrant. There can, however, be no justification for any further enlargement. Whatever care be taken to avoid vibration, the discharge of large volumes of smoke or hot air into the atmosphere constitutes a danger to the work of the Observatory which must necessarily increase with any further augmentation of the power. Hot air, smoke, and tremors of the ground are more or less unavoidable in a town, and an observatory in a town has, in the ordinary way, to take its chances. But a site near the Greenwich Observatory is the last place that should be chosen for the exceptional development of heat and power required in carrying out a scheme of distribution over the metropolitan area. Instead of further enlargement on this site, a suitable site should be chosen elsewhere for any extension that may be required, about which there should be no great difficulty, and the possible ultimate removal to it of the existing plant should be kept in view.

National Observatories seem to have troubles of different kinds. The following paragraphs from the Report of the Director of the U.S. Naval Observatory give food for thought:—

> After consultation with the District Commissioners, a bill was framed and introduced in the Senate, S. 5873, for the acquisition of the property known as the Industrial Home School property. The acquisition of this property is deemed very desirable in order that objectionable features now prevalent in that locality may be removed, and, incidentally, needed accommodations for observers may be obtained.
>
> The plat of 1732 acres, a part of the property known as "Normanstone," still remains to be acquired, and proceedings for its condemnation are before the court. It seems desirable that these proceedings be expedited and title to the property obtained.
>
> The Superintendent renews the recommendation of a former Superintendent that none of the property belonging to the Observatory tract outside the circle be sold.
>
> ### Estimates.
>
> In the department of astronomical observations there are large arrears of work on observations made, some of them as many as fifty years ago. The

reduction of these observations should be completed and the work of the Observatory brought up to date. With the present force of computers it is practically impossible to do this. An increase in the number of assistants in this department or an increase in the appropriation for miscellaneous computations is urgently needed.

OTHER observatories have troubles of other kinds. The benefits to be obtained from a fine climate at its best are so striking that the world in general is apt to forget the discomforts which often arise when the same climate does its worst. The following is an extract from a letter dated from the Lick Observatory on February 5:—"This has been a wretched winter here—we have had practically no observing since rather early in the fall, and the past two months have been almost one continuous storm, never clearing up between times. We have just had the worst storm (in some respects at least) in the history of the Observatory. Before one heavy snow had gone another set in, and we were snowbound for a time. For nearly two weeks no stage could reach the top—about five feet of snow here and badly drifted two-thirds of the way to Smith Creek; there the snow was about two feet deep, heavy enough to crush the stage barns. The headquarters for the stage change is Hall's Valley since. Our telephone and electric-power lines were almost completely wrecked and are not yet restored. No serious damage was done to any of the buildings or instruments. For a time fuel became so scarce that we had to eliminate a portion of the fires by combining; there was plenty of it a quarter of a mile away, but it could only be brought on men's shoulders."

Even from Southern California we have hints of similar troubles. Prof. Hale writes (on January 21):—" Our work is going on well in the valley, but tremendous storms have interfered seriously with observations on the mountain, where the snow is six feet deep on the level. It is disappearing rapidly, however, in the bright sunshine of the last few days (thermometer 72° yesterday noon in Pasadena). No such conditions have been known here in many years, and I hope they will not recur frequently." Many others will heartily join in this hope: six feet of snow, or even the five feet of Mount Hamilton, can be dispensed with cheerfully. Prof. Hale hopes to pay a visit to Europe shortly, arriving about the end of April, in order to be present at the Meudon Meeting of the International Union for Co-operation in Solar Research, which has now been fixed for Whitsuntide. It was hoped at one time that Mrs. Hale would accompany him, but this is not now found possible to arrange.

THE note last month on Herschel's long spells of mirror-grinding has elicited the remark that there was probably no particular need for continuous polishing, but simply that he worked in this tremendously enthusiastic way with a total disregard of health or bodily requirements. Certainly the following passage from 'Memoirs of Caroline Herschel' (p. 37) is capable of this interpretation now I look at it again. In reading it before I thought there must have been some particular device in polishing which needed continuous attention, but it is easy to see now that this may be quite a wrong interpretation :—

> For my time was so much taken up with copying music and practising besides attendance on my brother when polishing, since by way of keeping him alive I was constantly obliged to feed him by putting the victuals by bits into his mouth. This was once the case when, in order to finish a seven-foot mirror, he had not taken his hands from it for sixteen hours together. In general, he was never unemployed at meals, but was always at those times contriving or making drawings of whatever came in his mind.

SOMETIMES an aid to the memory, however trivial, is useful for making rough mental calculations. The number 57 is easily remembered to be (*a*) the number of degrees in the radian, (*b*) the number of minutes in the Moon's parallax, (*c*) the number of seconds in the coefficient of refraction ; but has it been generally noticed that it occurs also in re-converting degrees into radians, the value for 90° being 1·57 ?

A CORRESPONDENT writes that a copy of the following notice has been framed and hung up in the vestry of a church at Ramsgate :—

> NOTICE.
>
> In consequence of the Eclipse of the Sun occurring during the hours of Divine Worship on Sunday next, May 15th, St. George's Church will be closed in the afternoon of that day. (Signed) JOSEPH LOW, *Churchwarden.*
>
> May 14th, 1836.

The *Nautical Almanac* tells us that the eclipse was annular, though not central at Ramsgate. This simple device of a notice might have saved Horrocks some anxiety.

'THE History of a Voyage to the Moon : an exhumed narrative supposed to have been ejected from a lunar volcano' (London, Lockwood & Co., 1864). By good luck I picked up a copy of this work, of which I had never previously heard, at a second-hand book shop. It presents several features in common with later works of the same type, though coincidences of the kind might arise quite naturally. A clergyman finds in his garden the end of a chain attached to something which has penetrated deep into the ground, and which on excavation proves to be "a large ball of stone, in two parts firmly held together by bands of metal, apparently partly fused, to the thickest of which the chain was linked." On opening it he finds a thick MS. book in English, with letters in several tongues which commence as follows :—

> This Ball has been hurled from a Volcano in the Moon.
>
> We who have enclosed in it this letter and the accompanying MS. are two Europeans, who, having discovered a means both of leaving the Earth and of existing beyond its atmosphere, planned, attempted, and accomplished a voyage to the Moon—rising from the Earth in the neighbourhood of the Rocky Mountains, in the month of August, of the year 1859.

The letter further explains that the voyagers were unable to return to Earth themselves, as they had had an accident in alighting on the Moon and lost their car. This was built of a curious mineral ("repellante") found in the Rocky Mountains, which had the property of repulsion, except when shielded by metal. Considerable ingenuity was spent in building the car, which was a huge structure hundreds of feet in dimensions—none of your Jules Verne tiny projectiles—in which the occupants grew any quantity of trees and other vegetables. This garden was to supply them with oxygen to breathe, the simpler expedient of making the oxygen, used by Jules Verne, not having apparently occurred to the author. But there is a most interesting feature about this same supply of oxygen. In the projectile it will be remembered that the occupants forget to turn off the tap which supplies it and become wildly excited in consequence, indeed narrowly escape death. Now here is an entry from the diary of the voyagers of 1859 giving a corresponding experience:—

> *Fifth Day.* After dinner, Geister remarked that he thought our atmosphere . . . a little too bracing . . . and our spirits . . . somewhat unaccountably high. Inferred from this that the oxygen gas given out by the vegetation exceeded the quantity properly consumed by us. . . . We at once lit a fire outside "the house," and the atmosphere was gradually restored to its normal condition.

The scientific knowledge displayed is of the kind with which such works have made us familiar. Here is a passage on which students of astronomy might be asked to make comments:—

> By three o'clock in the afternoon [they started at 11.15] it appeared to us that we were at least 50 miles above the surface of the Earth, and, if so, had (according to generally received opinions) entered into space. We now placed the shields in such a position that the power of the repellante was turned in an oblique direction against the Earth. It was our intention to get over the ecliptic, and, when once directly under the Sun, to ascend perpendicularly in a direct line for that luminary, and thus intercept the Moon when in her monthly revolution she should come between it and the Earth. Up to this time we had continued almost directly over the spot whence we started, which, it is almost needless to say, was occasioned by the motion of the Earth round its axis being communicated to the Lunaviot as it rose from the ground, in the same way that a cannon ball, if fired from a cannon pointing upwards in an exactly perpendicular direction, might in falling, if no wind were blowing, return into the cannon's mouth, although, supposing one minute to be occupied in its ascent and descent, the cannon would in that time have moved many miles to the eastward.

When the voyagers arrive at the Moon they find the back of it inhabited by "some people of the Earth to whom another and perfect Material Existence is accorded, for that, through principle or affection, they had sacrificed worldly happiness, holding fast all that is morally beautiful and heroically noble"; and it is satisfactory to find that those chosen to enjoy this Special Existence include "scientific and literary men, artists, and others who have sacrificed to an intellectual ambition the material pleasures of life." The picture of Moon life is most attractively drawn, and ends with the journey taken to the Earth side of the Moon, in order to throw the stone ball into a volcano on the chance that it may reach the Earth: "in recollection of the belief entertained by astronomers that many apparently meteoric stones *really come from* the Moon."

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. APRIL, 1907. No. 382.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1907 March 8.

Mr. H. F. NEWALL, M.A., F.R.S., *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder. One hundred and eight presents have been received since the last Meeting, including the following:—Report of the Italian Commission on Observations of the Total Solar Eclipse of 1905 at Alcala Chivert, Spain (presented by the Catania Observatory); J. A. C. Oudemans, Mutual Occultations and Eclipses of the Satellites of Jupiter (presented by Prof. Nijland); Prague Observatory, 'Astronomische Beobachtungen, 1892–99, nebst Zeichnung, en und Studien der Mondoberfläche, von L. Weinek' (presented by the Observatory); Müller und Kempf, 'Photometrische Durchmusterung, General Katalog' (presented by the Potsdam Observatory); Astrographic Chart of the Heavens: 40 Charts presented by the Royal Observatory, Greenwich, and 20 Charts by the San Fernando Observatory.

The President. A petition has been received for signature from the Decimal Association urging the adoption of a decimal system of weights and measures in the United Kingdom. This petition lies in the Office of the Society in case any Fellows wish to affix their names.

The President then called on Mr. Cowell to open the proceedings.

Mr. P. H. Cowell. This paper on perturbations of Halley's Comet includes calculations of perturbations during the interval since the last return in 1835 up to the next return in 1910 for the action of Jupiter and the action of Neptune, and it also collects a certain amount of information about how far prediction has been realized in the past. For instance, de Pontécoulant predicted the date of the last return to within a week, but in the eccentricity he was not so fortunate. For some unknown reason the changes of eccentricity are not those that have been predicted. Another section of the paper is a change in the method of calculation. Of course this is a case of a problem of three bodies, and everyone knows that that cannot be worked out algebraically in finite terms. In the case of one planet perturbing another planet the solution in series can be obtained; in the case of two planets it is not so complicated a problem; in the case of the Sun disturbing the Moon, of course one knows it is a problem that takes ten years to work out; but in the case of a comet a solution in series is quite impossible owing to the huge changes of the comet's distance from the Sun.

To give some indication of actual facts. We know that Halley's Comet takes 76 years to go round its orbit. It takes something like 6 years to go from the end of the minor axis to perihelion, and again 6 years to go from perihelion to the other end of the minor axis, and the rest of the time (64 years) is spent in doing the outer half of the curve. There are obviously great difficulties about getting an accurate expression for the perturbations during the outer half of the orbit, simply because they oscillate so violently. During 64 years Jupiter goes five times round the Sun, and its action on the Sun—it will be remembered that we are finding the orbit of the comet relatively to the Sun—is first from one side and then from the other, going through five complete periods. In computing this there is a change of method. We throw the disturbing function into two parts, one of which can be integrated exactly—that is, we can get the action between one point and another in a finite form and therefore as accurately as we please. The advantage of the method consists in this: the other part of the disturbing function is exceedingly small for the outer half of the orbit, so that the quantities to be summed by quadrature are less than one per cent. of what they originally were.

Mr. A. C. D. Crommelin. I should just like to say that the whole credit for this conception is due to Mr. Cowell, for my work has been simply mechanical computation. I think there is no doubt of the great importance of his work in computing perturbations of a comet of very elongated orbit like this, and it reduces the labour of computing to about one-tenth of what it was before. And it makes practicable a scheme that I have long had in my mind, that is of working back the perturbations of Halley's Comet, to find out whether the identifications by Dr. Hind before the year 1378 (which is considered to be the earliest return whose identity is absolutely certain) were really Halley's. I think that is now a possibility, and I am glad to say I have got three gentlemen (two of them Fellows of this Society) to help me in carrying the research back; it could hardly have been done but for this new method Mr. Cowell has initiated. But it is impossible, I think, to estimate the work he has done until one sees it in print, when the definite integral will be put into tabular form.

For Jupiter we find it absolutely sufficient to work by the method of mechanical quadratures for the inner half of the orbit, and for the rest of the orbit to use this definite integral, which will be, perhaps, the work of ten minutes.

For Saturn we may have to go a little further out until the term called V_2 there becomes practically insensible; and as Saturn's distance is double Jupiter's, we have to go out further before the ratio of Saturn's distance from the Sun to the comet's distance becomes small.

For Uranus I do not think the new method has any advantage over the old; but in any case the labour of working the perturbations by Uranus and Neptune is very small.

About the action of the Earth and Venus there was something very unsatisfactory in the way Pontécoulant did that. He broke off these perturbations arbitrarily where the comet's distance from the Sun was, I think, about 3 units; but the terms of the perturbations are still quite large; they formed 5 significant figures in the unit he adopted, so it was obviously unsatisfactory to break off at that point. But now the remainder can be accurately got by this definite integral, and it will be possible to get the action of the Earth and Venus without any very great expenditure of labour. And this action is not at all to be despised; it may amount to two or three weeks in a revolution, and after carrying the comet back nine or ten revolutions, it may amount to a quantity large enough to make identification quite doubtful, so I think it is a great point that it is now possible to get the exact action of the Earth and Venus.

About the actual date of the return. Our paper is not yet absolutely finished, and we cannot give a definitive date yet; but I think if we say the middle of May 1910 for the perihelion passage we will not be more than two or three days in error. We hope to give a more exact date before long, but we could not finish the Earth and Venus perturbations in time for the present Meeting.

Mr. W. T. Lynn. If Halley's Comet was not seen before the year 1378, I should like to know where it was then. Could it have passed near Neptune and so been brought into the solar system by the attraction of that planet?

Prof. Turner. I would like to ask whether the computations refer entirely to the longitude, or whether they have any reference to the other elements of the orbit, such as its plane and the motion of the node in that plane, because perturbations of these elements played an important part in the case of the orbit of the Leonid meteors. I think the computations there needed were more in reference to the motion of the node. I do not know how far this simplification of method extends to other parts of the theory of perturbations of the comet.

Mr. Cowell. In answer to Prof. Turner, we have only at present considered four elements of the orbit, namely, mean motion, eccentricity, epoch, and the position of the perihelion. We have not yet considered the two constants defining the plane of the orbit. In answer to Mr. Lynn, I have not myself looked into the matter to see whether there was at any time an unusually near approach to Neptune.

Mr. Crommelin. The nearest approach of the comet's orbit to that of Neptune at present is 9 astronomical units, and it certainly could not have passed very near Neptune at any time within the last 4000 or 5000 years; we should want a very large motion of the node indeed to bring the orbit anywhere near Neptune's orbit. Certainly there has not been such a motion as that. I have tried to deduce the motion of the node from the observations beginning with that of 1378, and they give on the average an increase of longitude of the node, referred to a fixed point in space, of $1^{\circ}\cdot 2$ for each return, so that we require a great many returns to alter the node by something like 90°, which would be necessary to make the orbit intersect that of Neptune. I do not think, if Neptune has been responsible for the capture of Halley's Comet, that it has done so within historic times; it must have been at a remote epoch.

Dr. W. J. S. Lockyer showed some recent photographs of the Sun taken with the spectroheliograph at South Kensington. One of them showed the large spot of February as it returned to view at the east limb on March 6. The photograph showed an apparent black line in a vertical direction through the spot, which was thought, at first glance, to be a photographic defect, but it remained in the photograph taken on the next day.

Dr. Lockyer also showed a photograph of the landscape taken at Paris in which the Sun was obscured by a passing balloon, giving the effect of a total solar eclipse.

The President. The view of the two bands of latitude of the spot-zones north and south of the equator is very striking in these beautiful pictures from the side view one gets from this Chair. To have pictures of the Sun taken, as some of these were, at 10 o'clock this morning and shown at 5 o'clock, is surely a very remarkable performance.

Mr. Newbegin. I was interested to see the line in the spot that was just coming into view. I noticed it myself distinctly through the telescope, and I put it down to the fact that it might be a very wide spot very much foreshortened.

The President called on Prof. Turner for an account of two papers relating to variable stars.

Prof. H. H. Turner. I have recently had put into my hands the observations made by the late Mr. Baxendell, and have extracted those of U Geminorum for immediate publication for a reason already given in the case of Pogson's observations. Baxendell began observing in 1836, and I hope the valuable material accumulated by him may be soon published. It is nearly copied out in ledger form, but a good deal of work remains to be done on the ledgers. These observations of variables by other people (I am afraid I have never made any myself) which have come into my hands from several directions during the past few years have led to my considering the types of variable star-curves, and that brings me to the second paper, in which I hope will be found some interesting speculation as to the nature of these stars. Most variables of long period have a minimum falling later than midway between two maxima. But that is not the case for all such stars, though some people have an idea that it is. For some stars the minimum falls earlier than midway, though the cases are not so numerous. Of 31 light-curves (19 from the Rousdon observations and a dozen which have just come to hand in a splendid volume from Mr. Parkhurst) I will show two extreme cases. The two maxima are put together, and you see the minimum for T Cassiop. falls before midway, and for Z Cassiop. the minimum falls after; and you see the extreme case of lateness is greater than the extreme case of earliness.

You see other features of the curve, as this pause on the rise to maximum for T Cassiop., and the curvature for Z Cassiop. instead of coming straight up. Such features can be represented by harmonic analysis, and that method is useful for summarizing the information in a convenient form. Instead of drawing a set of seven curves of types intermediate between those just shown, you see the general change of type by these columns of figures.

There is a definite run in coefficients, and it impressed itself upon me that there was some connection between the light-curves, seeing they fell into sequence, and not an absolutely simple sequence.

The next point to be noticed is that the Sun falls into the sequence. I mentioned that before (see Mon. Not. lxiv. p. 549), but only tentatively, and I must say a word about how the Sun is included. We have just been seeing some beautiful pictures of faculæ, and the faculæ we could not fail to notice predominated over the spots, so we may consider the Sun's light greatest when the spots are greatest. [May I further illustrate the point by a digression and show some pictures which have come from Tortosa, in Spain, where Father Cirera has started a spectroheliograph at the Observatory of the Ebro ?]

It is not unreasonable to assume that our Sun (although the light varies to but a small extent as compared with the variable stars) may have yet the same type of curve, counting it as a maximum when there are many spots, or rather many faculæ. But on the last occasion the Sun was outside one end of the series; with Mr. Parkhurst's additions he has come right into the series. His curve is almost exactly like that of V Delphini.

Well, now, it struck me there might be—I will not say more than that,—there might be some secondary cause modifying a curve of permanent type, and I began to wonder what secondary cause we could assign, starting, let us say, from the Sun as a type, and it occurred to me that perhaps the *difference in aspect* of the Sun as viewed from different points should be considered. We see the Sun from a point nearly in the plane of its equator. What would happen if we looked at the Sun from the pole? Well, it is pretty easy to get a general idea of the effect.

This diagram shows the spots as we know them; beginning at a high latitude they approach the equator and come into better view. In high latitudes they are a good deal more foreshortened; as the cycle goes on the foreshortening is diminished, and not only the foreshortening but the absorption which we know darkens the limb. But now if we looked at the Sun from its pole, when the spots jump to high latitudes they would be enhanced in value; there would be no foreshortening (practically the foreshortening is not commenced for a considerable distance from the centre of the disc) and no absorption. But the maximum would suffer considerable foreshortening and absorption, and the end of the cycle would be so near the limb that I have left it out in exaggerating the effect. At intermediate aspects we get intermediate effects.

In the calculations made I would remind you that two factors were involved, one calculable and the other not. Foreshortening is calculable; absorption is unknown. So I thought it best to take the two curves we had at the beginning and see what conditions were needed to convert one into the other. The curves represent logarithms, and so multiplying the light by a factor means adding something to the ordinates of one curve to get the other curve, and I enquired what must be the nature of the variation of latitude of the spots or the faculæ to give this transformation of the one curve into the other.

If we take foreshortening alone the mean latitude of the spots would have to vary from near to the pole to very near the equator, and that is not a reasonable supposition. We cannot suppose that if the faculæ are going to be so intense they can at the same time look so very near at the pole at one time and near the equator at another; but directly something is added for absorption we get diminution in the range of latitude required, and it compares reasonably with that observed on the Sun.

This is therefore not a point we can pronounce upon definitely: even if we know the amount of absorption in the case of the Sun we do not know how far it is applicable to the other stars; but it is shown to be not unreasonable that the form of the light-curve is an indication of the aspect in which we view the star. If we view it from its equator the minimum falls later than midway; if we view it from its pole we get the minimum earlier than midway between maxima. Now up to this point this enquiry extends only for the 31 stars of which I had light-curves. There are not many stars of this kind with the light-curve fully determined.

Looking about for other material, I took from Chandler's catalogue the only element which is available for classifying stars in the same way, namely, the quantity called $M-m$ by Chandler. I found all stars fall within the extreme limits already studied, with one or two exceptions. It is possible these extreme cases will disappear when there is a better determination. That sounds rather like an accusation against Mr. Chandler; but I will show presently why I say this. I found that the list I had taken was practically representative of the whole stars, so that we might say at present it is probable that the stars will fall into those classes. The next point was that, assuming that this classification tells us something about the position of the stars' axes, it was natural to enquire whether there was any peculiarity in the distribution of these axes in space, *e. g.* to see whether they have any relation to the galaxy; and so I took the extreme "equatorial" stars and the extreme "polar" stars. The striking thing about it is that whereas the former are distributed in all galactic latitudes, the latter are absent from the neighbourhood of the galactic poles. Of course it is mere speculation at present, but the interpretation would be this—that the axes of the stars might be nearly parallel to the plane of the Milky Way, so that when we look towards its poles we can see equatorial stars, but no stars with poles presented to us.

Now I come to the point why I must not lay too much stress on this at present, and it lies in the nature of the material. The 31 light-curves I think are good material, but the simple differences between maximum and minimum I rather suspect. The point turned up in comparing the curves computed from harmonics with the actual observed curves in the case of the maxima. I found the maxima from harmonics did not agree with the maxima as observed. This was a little disconcerting, and on further examination I have been led to think that the observer's maxima have been fixed by a rule which I believe to be not absolutely vicious but worthless. The general rule is shown in this diagram, copied from a publication of Mr. Knott. Chords are bisected, and their middle points joined by a curve which is produced to cut the light-curve. I give reasons in the paper for stating that this rule is misleading, and I fancy that its use has introduced systematic error. I think the observers of the variable stars will have to calculate areas of curves and the centres of gravity and moments and cease attempting to assign maxima. It struck me at that point I had come to the same question with which Prof. Karl Pearson was dealing in the case of the error-curves; so I wrote to him on the subject, and I found there was roughly the same problem, but there was no obvious solution. The solution must be deferred, at any rate to another paper. Therefore I hope to return to this work at a future time. But at the moment I believe we shall have to suspend our judgment about the classification of the stars in this way until we have reconsidered the assignments of the maxima and minima and consequent classification of star-curves; but I feel hopeful, from the way the work has gone, that this idea of orientation of axes might give a working hypothesis for classifying these stars.

Mr. Newbegin. I should much like to thank Prof. Turner for his most interesting paper, which one could follow from the beginning to the end with very great ease. The manner in which he worked from the pole view to the equatorial view of the stars was wonderfully well done and I enjoyed it much; and I only hope he will go further and unravel the whole thing. I have often heard Fellows who sometimes go to sleep here say how awfully dry it has all been. I only wish some of them had been here to-night. I think they would go away saying that diagrams at any rate for once have been most interesting.

Mr. Lynn. I also have been much interested in Prof. Turner's ingenious paper, but there are some differences between the variations of the variable stars and the Sun. In the first place, the Sun period is very much longer, amounting to several years, and at the same time the change in the case of the Sun is so much smaller, that at the distances of the fixed stars a change such as this would not be perceptible.

Mr. W. W. Bryant. I should like to ask if any of the stars referred to were spectroscopic binaries, as in that case there would be an obvious difference between polar and equatorial views, since no star viewed from its pole could be detected as a spectroscopic binary.

Mr. Maunder. I was very much struck with Prof. Turner's ingenious suggestion. Speaking for myself, I have been so accustomed to the position from which we look at our Sun, namely, from the plane of its equator, that it had not occurred to me to consider what would be the effect of looking at it from an altogether different direction, as from above one of its poles. It is quite clear that that would apparently modify the form of the sun-spot curve.

With regard to the suggestion that some determination might be obtained of the degradation of the Sun's light near the limb by taking photographs in which the images overlap to a greater or less degree, it so happens that I have a slide with me which shows such an overlapping image. The original photograph was taken on February 10 of the present year. On that day there were no fewer than four separate spots or groups of spots distinctly visible to the naked eye, an occurrence which, I think, is without precedent in my experience. Measures have been made of the diminution of the light towards the Sun's limb by Profs. Pickering, Langley, and Vogel, and a table of the results obtained by Vogel is given in Young's 'Sun' (p. 281).

Sir David Gill. With reference to the formulæ on the board, I would just like to ask Prof. Turner whether there is sufficient accordance with observation as found by using only three harmonic terms. Beyond that I would say that all previous reasons for the publication of all observations of the magnitude of variable stars are redoubled in importance by the considerations suggested by Prof. Turner's paper. Dr. Alexander Roberts, who has just returned from England to the Cape, tells me he has nearly completed his magnificent series of many thousands of observations of variable stars, and I will impress upon him the necessity of letting everybody know all particulars of every observation he has made. He is endeavouring to derive all sorts of equations from them, particularly from the variables with two components, to determine their diameters, a research rather different from that dealt with by Prof. Turner.

Professor Turner. It is quite true that the range of variation of light in the Sun is very small compared with that in variable stars; in fact we hardly know which way it goes; it is a rudimentary variation, and of course there are great dangers in working from such a rudimentary case where the range is small and the period very long. But I was so struck with the way in which the Sun did fall into the series that I went on. With regard to the spectroscope, it did occur to me to wonder whether we might possibly test the theory by spectroscopic observation. If the equator of a star was presented to us then there ought to be the approaching and receding of the limbs, and we should get a widening of the lines; but if the pole were presented, then there would be a narrower line. So I wrote to Sir William Huggins to ask whether he thought the observation could be made; but he did not think it was promising because the limbs would probably be a good deal obscured, but Sir William Huggins expressed himself interested in the general idea.

With regard to a question by Mr. Hinks, the tendency has been in times past to publish only the maxima—very seldom even the minima. In Chandler's catalogues you may get $M-m$ inferred from 20 maxima and 1 minimum. That gives an element of uncertainty in the work at present; and I would throw great weight into the scale in the way of trying to persuade observers to publish *the whole of their observations* and not merely a selection. I hope we shall get what material is lying unpublished published, and I hope we shall get material for determining the whole curves.

I am obliged to Mr. Maunder for his reference, which I ought to have known of.

Sir David Gill raised a point which I might refer to: did I get good enough accordance with three harmonics? The fourth harmonic in most cases, in all the cases I tried, came to less than $\frac{1}{10}$ of magnitude.

The President. We have to accord our warm thanks to Prof. Turner for an exceedingly interesting paper. He has already in late years put us under obligation, first by preparing Mr. Knott's observations for press and then by editing the Rousdon variable star results, and so giving us two volumes of Memoirs of observations in this line of work, and now he has tackled effectively the question of the cause of variability in a certain number of variable stars whose light-curves have been accessible to him and has tried to organize the results in such a way as to show that the cause of variability may be perhaps similar to that which is exhibited by the Sun in virtue of faculæ and sun-spots. There seem to be so many points of interest to attack that I hesitate to ask about any of them. I have already privately asked Professor Turner whether there are any special spectrum phenomena attaching to these stars. If the stars he has taken are of the long-period type with which Mrs. Fleming's work at Harvard College has made us familiar, spectroscopic examination would show that their maxima are connected with a marked outburst of bright hydrogen lines; and that might lead one to think that a study of the mode of variability of the prominences on the Sun will come to be of special interest in the continued study of these variables.

As there were no more papers suitable for discussion presented at the Meeting, at the request of the President *Sir David Gill* gave the principles of a paper that he is preparing for a subsequent meeting. This is an investigation into the question of variation of personal equation, in transits with the magnitude of the star observed in the two cases when a recording micrometer was used, and secondly when the stars were observed in the ordinary way over the fixed wires of a transit instrument, the method of research being to choose stars of the same declination and to measure the distance between them first by transit and then by heliometer measurement, and then compare the results *.

The following papers were announced and partly read:—

R. T. A. Innes. "Computation of Secular Perturbations."

Rev. L. A. Williams. "Observations of Occultations."

"Baxendell's Observations of U Geminorum." Edited by *H. H. Turner.*

H. H. Turner. "On the Classification of Long-period Variable Stars, and a possible Physical Interpretation."

P. H. Cowell and A. C. D. Crommelin. "Perturbations of Halley's Comet."

Royal Observatory, Greenwich. "Observations of Minor Planets from Photographs taken with the 30-inch Reflector of the Thompson Equatorial during the Year 1905."

Rev. T. E. Espin. "A New Nebula."

The following were elected Fellows of the Society:—

Edward George Bloomfield Barlow, Ditton Lodge, Stourwood Avenue, Bournemouth.

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* Sir David Gill's address has not been given in full, as his paper on this subject will probably be summarized in the report of the next Meeting.

Lieut. F. G. Cooper, R.N.R., H.M.S. 'Ocean,' 131 Sutton Court, Chiswick, W.

Edward Power, F.S.A., F.G.S., 16 Southwell Gardens, S.W., and Watership, Newbury, Berks.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

SION COLLEGE, VICTORIA EMBANKMENT.

Wednesday, 1907 February 27.

F. W. LEVANDER, *President*, in the Chair.

Secretaries: J. G. PETRIE.
J. A. HARDCASTLE.

MR. PETRIE read the Minutes of the previous Meeting, which were confirmed.

Mr. Hardcastle read the list of presents received, and the thanks of the Association were accorded to the respective donors.

The names of five Candidates for Membership were read and passed for suspension, and the election by the Council of nine new Members was unanimously confirmed.

Mr. Hardcastle gave a *résumé* of the Interim Report of the Variable Star Section by *Col. Markwick*, dealing with the light-curves of six short-period variables, viz., T Monocerotis, ζ Geminorum, η Aquilæ, S Sagittæ, T Vulpeculæ, and δ Cephei. Twenty-one Members of the Section had taken part in the observations, which numbered 2865. The laborious work of reducing these observations had been undertaken by Mr. C. L. Brook, F.R.A.S.

The President announced that the Report of the Rousdon Observatory had been received, and would appear in the 'Journal.' It dealt with the observations of long-period variable stars during the year 1906, but included also cometary and planetary observations.

Mr. Gavin J. Burns read a paper on "The Proper Motions of Stars." The writer said that the new edition of 'Groombridge's Catalogue of Circumpolar Stars' furnished a large amount of fresh material for the study of proper motion. He had abstracted from the Catalogue the proper motions of all stars of the fifth magnitude or brighter. There were 261 of these stars, and, excluding 14 of exceptionally large proper motion, the average annual proper motion was $0''\cdot08$. In the same way he had abstracted the proper motions of all stars of magnitude 8·5 and fainter. There were 291 such stars in the Catalogue. After excluding one star of over $0''\cdot5$ the average proper motion of the remaining 290 came out at $0''\cdot04$ per annum. As the real motion of stars in space must be independent of the magnitude, it follows that, if they took an average of two or three hundred stars, the average proper motion must be proportional to the average parallax. It consequently appeared that the average distance of the second group of stars was about double that of the first group. The average magnitude of the first group was about 4, and that of the second group about 9, or there was a difference of five magnitudes in the two groups. But the light ratio corresponding to five magnitudes was 1 : 100, and the corresponding ratio of distance was 1 : 10. The inevitable conclusion appeared to be that faint stars were, as they appeared to be, of much less intrinsic brilliancy than bright ones, and that they do not appear fainter merely on account of their greater distance.

Mr. Crommelin then delivered a lecture on Halley's Comet. Having said that it was a great pleasure to him to do something in the way of celebrating the memory of the great English astronomer whose name was for ever associated with this famous comet, for at the time of its appearance in 1682 there was probably no one else in the world who was able to do the brilliant piece of work which Halley carried out, Mr. Crommelin proceeded to give the history of the observation of Halley's Comet and of its identification with comets seen at different dates, showing by means of a table the similarity of the elements of the orbits of these comets, and quoting Dr. Galle's opinion that the comet of 1378 was the earliest apparition that could be considered certain. The lecturer then proceeded to explain the effect of the different planets in perturbing the orbit, and described the work that Mr. Cowell and he were doing in this direction *. He also said that as the orbit of the comet passed very near the orbit of the Earth, and as meteors were probably spread out to a certain extent on each side of the orbit, it was quite likely that the Earth might come across them.

The President said he was sure he was acting as the mouthpiece of those present when he said they owed a deep debt of gratitude to Mr. Crommelin for having so kindly put before them the results of his researches with respect to Halley's Comet. They would all look forward with interest to the settlement of the question whether the comet of 1066 was Halley's or not.

Mr. C. J. Whitmell read a paper on "Contact Times of the Eclipse on August 30, 1905."

The President said there were other papers which had been communicated, but there was not sufficient time to read all of them. Mr. Gheury had sent one on "The Aurora and Sun-spots," following upon the letters which appeared in the last number of the 'Journal' regarding the aurora of February 9 last. There were also some photographs of sun-spots taken by Mr. Newbegin, and Mr. Maunder had brought a photograph of sun-spots, and one dealing with the magnetic storm on the day mentioned which would be exhibited at a future Meeting. Mr. Hardcastle's second paper on "The Tide on the other Side," being mainly mathematical, did not admit of being read, but would appear in the next issue of the 'Journal.'

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* See p. 151.

The Meeting stood adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday evening, March 20, at the Institution of Civil Engineers, Great George Street, Westminster, *Dr. H. R. Mill*, President, in the Chair.

Major B. F. S. Baden-Powell gave a lecture on "The Exploration of the Air." He began by saying that the atmosphere forms a vast ocean above us, an ocean but little explored. We crawl about the ground like crabs on the bottom of the sea; we make our meteorological observations down on the ground, ignorant of all that is going on in the midst of that great expanse of air above our heads where the clouds hang about, where the rain and the hail are formed, where the lightning-flashes have their origin. We look to the cross on St. Paul's or the top of the Eiffel Tower as the most exalted of points at which to observe the mechanism of the atmosphere; yet these compare to the aerial ocean but as a pin stuck in the carpet does to the air of a room. If all the clouds in the sky were suddenly solidified in their various shapes and different elevations, what vast realms of unexplored territory there would be. But even though not solid land, there is still plenty to explore in these regions of everchanging cloudland. If we can mount up among them, we can ascertain the temperature, the humidity, the air-pressures, the electrical effects, and many other points on which we are at present very ignorant.

Major Baden-Powell said that there are two classes of people interested in the exploration of the atmosphere: (1) the Meteorologists, who study it chiefly to find out about the weather; and (2) the Inventor, who would utilize it as a highway of travel. But these two are by no means rivals. The attainment of their objects will be of mutual assistance to one another. The aerial navigator will want to know all about the currents and the conditions of the air, while the meteorologist will derive the utmost benefit from the ability to visit any parts of the atmosphere. There are three means now at the service of man by which he may ascend into these desirable regions, or may send up self-recording instruments to probe the mysteries of the skies, viz., balloons, kites, and flying-machines. Although at the time of its invention the balloon was hailed with acclamation as promising the conquest of the air to man, yet it is now realized that this cumbrous and delicate apparatus is not capable of much practical application. It is nevertheless useful (1) as an observatory for scientific investigation, (2) as a means of reconnaissance in war, and (3) as a most agreeable way of spending an hour or two in blissful peace and sublimity. But recently great strides have been made in the improvement of the balloon in the way of providing it with engines and propellers so that it may be driven to any predetermined goal. Twenty-five years ago the French Government made the first dirigible airship, and now they possess one, if not more, that seems to be a really practical air vessel of war. Count Zeppelin in Germany has also produced a machine which in point of size as well as in speed has beaten all records. Going to the other extreme, we have small balloons now capable of attaining the greatest heights carrying self-recording instruments. Such contrivances have recently ascended to the enormous altitude of 82,000 feet, or nearly 16 miles, above the surface of the earth. Closely connected with this subject of *ballons sondes*, as the French call them, is that of meteorological kites. These also have been much improved in recent years, and instruments lifted by kites retained by steel wires have actually ascended to a height of 4 miles. Kites of a much larger dimension have also come into use during the last few years. At Aldershot they have been regularly introduced into the service. Men were first lifted by this means in 1895, in which year the lecturer made a number of ascents up to 100 feet high, but improvements have gradually followed until now men have actually gone up to a height of 3000 feet, an elevation practically beyond the reach of rifle bullets, and so high as to render the aeronaut almost invisible.

Major Baden-Powell, in conclusion, referred to a subject which, if it has not hitherto had any very practical results, yet promises to bring about perhaps more extraordinary changes in the life of man than have resulted from any other of the marvellous inventions of the 19th or 20th centuries. The flying-machine has come, and has come to stay. Years ago people argued that it was impossible to lift in the air that which was far heavier than the air. But the advocates of the cause pointed to the birds, and there was no gainsaying that the thing was possible. Then the pessimists cried that it would not be possible to balance the apparatus without the intelligence of a bird; but models were made, finally so good as to fly for three-quarters of a mile. Then men took to gliding on wings and sailing on the wind. Huge machines were constructed with very light engines, and at last, during the last two or three years, not only have men been successfully raised off the ground, but have been able to sustain themselves in the air for half an hour at a time. Very little more remains now to be done before we can say that man has veritably conquered the air.

Accounts of the Great Fireball of 1868, *October* 7, *showing it to have undergone a strong deflection of its Real Path.*

In lately reading over a beautifully written out MS. by the late Mr. W. E. Besley of a lecture on meteors delivered at Crouch End in about the year 1899, the interesting details there given to illustrate the brilliant phenomena of large fireballs, of the exceptionally fine display of brightness presented by "one of the most magnificent fireballs of recent years," as the instructive lecture very rightly describes that of $11^h\ 50^m$ P.M., October 7th, 1868, for it indeed well deserved the title, recalled to my mind the much-regretted suppression in 1870, owing to its length, of the "Luminous Meteor Committee's" catalogue of observations, by which some excellent descriptions of that fine fireball by good observers had the mishap to remain unpublished, and its real path in space to have never received the amount of careful investigation which the recorded path-accounts were found to need for its correct determination. Besides having made one such attempt in 1898–99, I have now again sought out and studied the existing records for the purpose of quite independently conning out from them, once more, a fairly certain determination of the fireball's real path.

Fourteen descriptions of the meteor, four of them from France and Belgium, and the rest in England, and all but four or five giving either no indications at all, or totally mistaken ones of its apparent course-direction, are contained in the British Association Report for 1869. From those few clear, but largely incomplete and not too well accordant path-accounts—at Sandown (I. of Wight), and Wolverhampton *, Paris, Angers, Leytonstone (or Wanstead) and Walworth †, London, added to better-noted path positions supplied to him from Birmingham, and by an experienced observer at Clifton (Bristol), Mr. G. F. Burder,—Mr. W. H. Wood attempted to obtain an approximate determination of the meteor's real course, and concluded it to have been about 170 miles in length, described in about 5 seconds, from about 90 miles above a point ten or twelve miles onwards from Avranches, Manche, across Calvados, to little more than ten miles above a point of the

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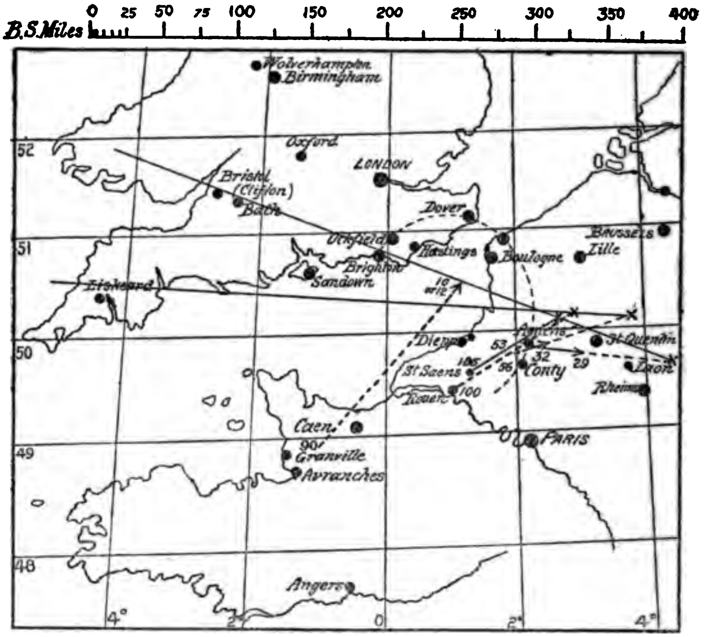
* The course noted as "from South to North, disappearing near the north horizon," was actually directed from N.W., and ended near the S.E. horizon, as accounts affirmed quite correctly from the neighbouring town of Birmingham. But other path particulars supplied from Wolverhampton, as that of the path's apparent length, recorded there, were yet of very useful value. At Sandwich (Kent), in Brussels, and at Conty (Somme), equally erroneous estimates were noted of the apparent position and direction of the meteor's course.

† By "Cassiopeia's Chair" (which was then just overhead), the asterism of Orion was doubtless intended to be specified at Walworth (London) as the constellation "to the right" of which "the meteor descended," since at all the south-eastern English stations at which the meteor was observed, its descending course was described as ending a little to the right of Rigel.

English Channel about 30 and 40 miles distant southwards, respectively, from Boulogne and Hastings. But it is yet clearly obvious from a note extracted by Mr. Wood from a French account of the meteor's appearance at Rouen, that it appeared there "to fall to earth in the direction of St. Hilaire," the extreme eastern or very slightly north of eastern suburb of that city, that the meteor took a far inland course over the north of France instead of a widely sea-going one over the English Channel. The concluded radiant-point, very near the equator about in R.A. 337°, seemed naturally to suggest that the meteor's path-direction was derived originally, with some considerable deflection from that of the noted autumn shower of "α Pegasids," Mr. Greg's general radiant-centre T_1 in the British Association 'Meteor Atlas' of 1867, at about 339° + 13°. But the low-concluded declination of the radiant-point rested upon estimations of the meteor's apparent slope of path, "about 45° from vertical" at Sandown and at Clifton, whereas path-notes at both Bath and Wolverhampton, among those collected and used by Mr. Wood, of the same



Note.—The dotted Path-projections are those obtained by Mr. Wood and Prof. Herschel in 1868–69 and 1898–99. The full-ink Path-mapping is that arrived at in the present paper.

terminal part of the flight as that seen at Clifton, stated that the intensely bright red fireball, which proceeded from the first extremely luminous white flash, appeared to fall almost perpendicularly.

The suppressed Catalogue's, and some later Observation-data.—It was not until a solicitation from Mr. Denning, in the winter of 1898–99, led me to submit to a careful revision all the many real meteor-paths contained in the British Association 'Meteor Reports' of the years 1860–70, that the several new good observations of this interesting fireball which had been left neglected in the unprinted Catalogue of the 'Report' for 1870, were sought out and used in conjunction with the above-named earlier ones to obtain as near an approximation as possible to the height and locality and to the speed and direction of motion of the fireball's real path. There were twelve or thirteen new descriptions,—two of no path specifications at Lille and Paris, but a later added, rather usefully path-locating one at Rheims, in France; with two in London and three at Greenwich, the latter three devoid of path-accounts; one equally and one nearly as devoid of path particulars at Horsham and at Hastings, and three which transcended all the others most pre-eminently in path-recording details at Dover, Brighton, and Uckfield, in Kent and Sussex. The two accounts in London, making the brightest closing part of the flight descend to left at about 40° from horizontal, to a low south-easterly altitude of 10°–20°, by bearings about E.S.E. or S.S.E., respectively, of noted near or distant buildings, and again the course given by streets in Rheims by Dr. Le Cler of that town, in 'Kosmos,' assigning it a motion northwards at an altitude of about 30° in the end part of the flight, skirting the tops of a long range of house-roofs almost horizontally from about 20° to about 40° N. of W., severally afforded corroborative evidence that the terminal portion of the real path had been correctly laid down in the here computed end or ember stage from the clearer and stricter observations, since that descending path stage would have appeared, in London, to be sloped 40° from vertical, from 19° to 9° in altitude, at 49° to 40° S. of E., and at Rheims almost truly horizontal, at altitude 29°, proceeding northwards from 25° to 37° N. of W. But coupled with the doubtless nearly perfect course by stars recorded by M. Tremechini in *Comptes Rendus*, from his optical workshop on the heights of Belleville, near Paris, as from α Cephei to η and to between β and γ Ursæ Minoris and thence to some considerable, but, as it would seem, too bewilderingly spark-encompassed distance further to have been accurately noted, and with the well-observed apparent place which Mr. Burder secured of the terminal 10° of the flight at Clifton, both borrowed from the earlier collection, the apparent near proximities of the meteor's course to Jupiter, Rigel, and some other visible fixed stars, recorded at the above three places, Dover, Brighton, and Uckfield, were really all the records made that were definite and complete enough to fix the meteor's real path with some precision.

The three latter contributions were furnished, that at Dover in a letter from a young military officer stationed there, Mr. E. H. Curling, to the then Astronomer Royal, the late Sir G. Airy, describing the apparent track as by his eye-estimation about 20° long, in 2 or 3 seconds, from 15° E., or left of Jupiter (then at about altitude 40°, 7° W. of S.), to 10° W. or right of Rigel (then at about altitude 10°, 26½° S. of E.*). The next, at Brighton, was a communication on the subject of the meteor in the 'Express' by a well-skilled astronomer, Mr. H. C. Malden, who, while studying with a telescope the constellation Taurus, saw the meteor "begin close to *ξ*, *ο* Tauri, exactly at 45° + 10° [altitude 42°, 48½° S. of E.], and descend almost perpendicularly towards about 35° S. of E., disappearing just to the right of Rigel." Were this assigned horizon bearing towards which the meteor seemed to fall, at Brighton, perfectly correct, the meteor would have disappeared just 10° to the right of Rigel, but were the words "just to the right of Rigel" to be really deemed to signify a distance as large as that from that star, it will presently be seen that this and the recorded end-point bearing at Dover and Clifton would all three together be in a quite intractable predicament of irreconcilable discordance.

From Uckfield, about 15 miles N.E. from Brighton, Mr. C. L. Prince wrote to Mr. Glaisher that the meteor "fell almost perpendicularly from very near the Pleiades" [at 55° + 25°, or altitude 48½°, 27° S. of E.; but by the relative position of Uckfield from Brighton, necessarily a little further south and west, or towards the right, and in rather less R.A. than Mr. Malden's "certain" starting-point at 45° + 10°, which was 48½° S. of E. and already about 15° of arc westward from the Pleiades !], and disappeared when "just opposite to" [meaning, doubtless, when just horizontally, or else squarely in its course, abreast of] "Rigel." But here again, since it was necessarily further west or to the right of Rigel than Mr. Malden's view of the extinction which his note suggests might be as much as 10° to the right or westwards from that star, nothing to supplement but only to generally substantiate the Brighton path description, except that it pretty distinctly indicated decidedly close nearness of the disappearance-point to Rigel, was really furnished by the vague outlining of the close and the starting points of the apparent course supplied by Mr. Prince's note at Uckfield.

Apparent incompetencies of the observed apparent end-points to fix the Meteor's real place of disappearance.—It is remarkable that although Dover is nearly 70 miles E.N.E. from Brighton, yet there Mr. Curling estimated the end-point of the meteor's flight to be, just as Mr. Malden affirmed it in effect to have been at

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* These and the following altitudes and bearings are all referred to the horizon plane of a place on the meridian of Greenwich, in N. latitude 50½°, as about the mean place on the globe of the widely dispersed principal observing stations.

Brighton, still only 10° to the right from Rigel. But nearly 30° of displacement due to parallax between Dover and Brighton must be allowed, and the latter station's view and that at Uckfield, of the end-point must be placed as near to Rigel, and apparent place of disappearance at Clifton as extremely " little south of E.S.E." as possible, to confine the real end-point to no greater distance from those English stations than about the neighbourhood of Amiens, on the north meridian of Paris, where M. Tremechini's description by stars of the meteor's apparent steep descent from the left, to its surprising concluding deflagration there, nearly due north, undoubtedly located it.

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lines of sight to the real end-point will continue still to be nearly 30° as in the first made supposition. In the second, evidently in the main fictitiously made assumption, it thus appears that Mr. Curling's account of the apparent end-position should have been about 41° instead of about "10° to the right, or west from Rigel," a result which, like the previous one of 31° to the right from Rigel, is quite outside the capabilities of even a liberal allowance of probable error in the Dover observation to account for.

With supposed straight-forward prolongations of the observed course by stars in Paris, to 18° and to 9°, respectively, beyond the explosion-point in Ursa Minor, the concluded real point of disappearance on the first of the two above supposed presumptions would be at 25 miles above a point 8 miles E.N.E. from Amiens—or 4° E. of N., altitude 18°, and 5 miles east of 74 miles north from Paris,—and that afforded by the second supposition (that the Brighton end-point's bearing should be 10° further to the right than that adopted in the former case), would be at 26 miles above a point 23 miles due south, or very nearly due south, from Amiens,—or 2° W. of N., altitude 27°, and 2 miles west of 51 miles north from Paris,—both of these two real point positions having nearly the same amounts (about 30°) of Brighton-Dover parallax allotment with the two points of the above two illustrations, from lying both of them nearly on the circumference of the same geometrically traced circle. But, for that reason, neither of them, it has been seen, can be regarded as a true position of the meteor's real point of disappearance, owing to the thorough lack of good agreement which they both disclose with the well-recorded apparent end-point of the good eye-estimation of the track at Dover. The second of the two ambiguously noted and therefore here alternatively submitted real end-points—the one far south of Amiens—must evidently be at once discarded, because in the first of the two named Brighton bearing lines' already more simply obvious qualification for adoption, it may fairly be said to add a further 10° gratuitously to the already irksomely enough outstanding deviation of 20° which has to be explained away, of the Dover observer's estimate of the end-point's direction from those of the observers at the other English stations.

Explanation of the unadaptability of the end-point observations to each other, by an explosively produced, extremely large deflection, in mid-journey, of the Fireball's real path.—Both in a similar solution of it which I obtained in 1899, and in my present quite *de novo* attack on the real path's construction, it required weeks and months of careful study of this strange feature in the interesting descriptions of the meteor, to disclose at last the existence, accidentally, in their combination, of the remarkably simple and straight-forward method of proceeding (foregoingly invoked and first applied, above, to the effectual deletion of some portion of the inordinately large discordance), for pointing out at once the most complete and effective possible means of lessening the remainder, and of freeing the real path construction as much as may be possible from the serious inconsistency with which it is still very obviously and obtrusively affected.

The above imaginary circle supposed to be traced through Brighton, Dover, and Amiens, not only assigns the direction of translation (17° W. of S.), along which the real end-point's Brighton-Dover parallax, or angle of divergence from each other of those two stations' lines of sight towards the disappearance undergoes no alteration, while all the English lines revolve in bearing, in a body towards the right, or south-westerly away from Rigel; but it also obviously prescribes, in fact, a direction transverse to the latter one, or towards 17° S. of E., as that along which the same parallactic angle would decrease most rapidly; while the end-point's sight-lines at Brighton and Clifton would in any such recession of the real point to more or less remoter distances than Amiens from the English stations, not change their bearings and altitudes appreciably in consequence, from the adopted apparent disappearance-points' directions, respectively, at those stations. They would remain almost motionless at "just" (*i. e.* one or two degrees) "to the right of Rigel," at Brighton, and by 2° or 3°, only, "a little south of E.S.E.," at Clifton. If this latter Brighton line of sight is accepted as a true guide-line to the real termination, then by totally rejecting the presumed straight-onward extension of the observed track in Paris beyond the point about between the two "guards" of Ursa Minor where the already stately fireball broke out into what M. Tremechini termed "*une féerie indescriptible*," and "*un cône immense*" of flaming bodies accompanying the still splendid head, we may quite certainly assume the remaining path-section of the fireball's course onwards to have been strongly deflected at the outburst to a long way eastward of the neighbourhood of Amiens; and that instead of at the above found height of 25 miles over 8 miles E.N.E. from Amiens, towards which extinction on the same Brighton guide-line the meteor's path appeared in Paris to be descending steeply, a real *locus* of the globe's extinction may be more confidently chosen 35 miles E.S.E. from this and 40 miles eastwardly from Amiens, near and south-westwards from St. Quentins, Aisne, where at a height of 29 miles the deflected meteor-globe—"at first bright red and thereafter blue, yellow and then green"—would be finally extinguished 35 miles east of 62 miles north, and at altitude 22°, 29° E. of N. from Paris.

The apparent flexures of the path by the violent disruption would reach, at all the English stations, no greater angular amounts than 4° or 5° to 12°–14°, and would escape notice in the outburst's extensive enveloping bright spark-cloud. But much more extraordinary deflections, as will be shown hereafter in a Table, would present themselves at Continental stations, although, even there, only a few of the best accounts appear to have distinctly noticed them. The extinction's distant transportation thus in the direction of fastest decrease of the Dover-Brighton parallax, effects a material retrenchment of nearly 8° in the no-otherwise importantly reducible outstanding divergence of 21° in the Dover statement of the termination's bearing from other English accounts of its position, and the remaining discordance of 13° is, indeed, further reducible, if the sloping path-line's nearest lateral distance when prolonged, from Rigel, may have been intended to be signified by the description; for the swervingly amended course thus substituted for a straight end-portion of the flight, makes that shortest distance to be only 15°, exceeding the judged distance by only 5°, where, on nearly reaching the horizon, the prolonged path would make its closest possible approach to Rigel.

A. S. Herschel.

[To be continued.]

The Albedos of the Planets.

As the mass of the brighter component of the binary star *α* Centauri is equal to that of the Sun, and its spectrum the same, we may consider it to be a duplicate of our Sun, and it has occurred to me that we might estimate the albedos of the superior planets by comparing their photometric magnitudes when in opposition with that of *α* Centauri.

In my paper in the 'Monthly Notices R. A. S.' for January 1903 I have shown that, taking the parallax of *α* Centauri at $0''\cdot75$, and assuming that the diameter of the brighter component is the same as that of our Sun,

$$\left.\begin{array}{l}\text{Apparent bright-}\\ \quad\text{ness of Sun}\end{array}\right\} = \left\{\begin{array}{l}75{,}232{,}650{,}000 \text{ times that of the}\\ \quad\text{brighter component of } \alpha \text{ Centauri.}\end{array}\right.$$

Let

d = diameter of planet in miles,

D = Earth's mean distance from the Sun,

rD = planet's mean distance at opposition,

a = planet's albedo,

m = photometric magnitude of planet at mean opposition,

we have

$$\text{Area of planet's disc} = \frac{\pi}{4}d^2,$$

$$\text{Area of hemisphere at planet's distance} = 2\pi r^2\mathrm{D}^2.$$

Hence portion of Sun's light intercepted by planet

$$= \frac{a\pi d^2}{8\pi r^2\mathrm{D}^2} = \frac{ad^2}{8r^2\mathrm{D}^2}.$$

Hence we have the formula

$$\frac{ad^2}{8r^2D^2}\cdot\frac{1}{(r-1)^2}=\frac{1}{75{,}232{,}650{,}000\times b},$$

where

$$\log b = 0{\cdot}4(m-0{\cdot}31) = 0{\cdot}4m - 0{\cdot}124$$

(0·31 being the photometric magnitude of the brighter component of α Centauri as measured at Harvard).

Taking the logarithm of both sides of the above equation, we have

$$\begin{aligned}\log a + 2\log d - \log 8 - 2\log r - 2\log D - 2\log(r-1)\\ = \log 1 - \log 75{,}232{,}650{,}000 - \log b,\end{aligned}$$

and

$$\begin{aligned}\log a &= \log 8 + 2\log r + 2\log D + 2\log(r-1) - 2\log d\\ &\quad - \log 75{,}232{,}650{,}000 - \log b,\\ \log a &= 0{\cdot}9030900 + 2\log r + 2\log D + 2\log(r-1)\\ &\quad - 2\log d - 10{\cdot}87640640 - (0{\cdot}4\,m - 0{\cdot}124),\\ \log a &= 0{\cdot}9030900 + 2\log r + 15{\cdot}9350960 + 2\log(r-1)\\ &\quad - 2\log d - 10{\cdot}87640640 - 0{\cdot}4m + 0{\cdot}124,\end{aligned}$$

or $$\log a = 6{\cdot}0857796 + 2\log r + 2\log(r-1) - 2\log d - 0{\cdot}4m.$$

Let us apply this formula to each of the superior planets.

Mars.—Here $$r = 1{\cdot}52369\,13,$$
and therefore

$$r-1 = 0{\cdot}52369\,13.\quad d = 4332 \text{ miles (Barnard)},$$
$$m = -1{\cdot}75 \text{ (Newcomb)},$$

and we have

$$\log a = 6{\cdot}0857796 + 0{\cdot}3657940 + \bar{1}{\cdot}4381638 - 7{\cdot}27337\,68 + 0{\cdot}700,$$
$$\log a = \bar{1}{\cdot}3163606, \quad\text{and}\quad a = 0{\cdot}2072.$$

Zöllner found 0·2672, and Müller 0·22.

Jupiter.—Here

$$r = 5{\cdot}202800,\quad r-1 = 4{\cdot}202800,$$
$$d = 87{,}380 \text{ miles},\quad m = -2{\cdot}23 \text{ (Newcomb)},$$

and we have $a = 0{\cdot}595$.
Zöllner found 0·62, and Müller 0·62.

Saturn.—Here

$$r = 9{\cdot}538861,\quad r-1 = 8{\cdot}538861,$$
$$d = 73{,}000,\quad m = 0{\cdot}88,$$

whence $a = 0{\cdot}6744$.
Zöllner found 0·52, and Müller 0·72.

Uranus.—Here

$$r = 19{\cdot}18329, \quad r - 1 = 18{\cdot}18329,$$
$$d = 34{,}900 \text{ miles}, \quad m = 5{\cdot}75 \text{ (Newcomb)},$$

whence $a = 0{\cdot}61$.

Zöllner found 0·62, Müller 0·60.

Neptune.—Here

$$r = 30{\cdot}05508, \quad r - 1 = 29{\cdot}05508,$$
$$d = 32{,}900 \text{ miles}, \quad m = 7{\cdot}84 \text{ (Newcomb)},$$

whence $a = 0{\cdot}6276$.

Zöllner found 0·46, and Müller 0·52.

Dublin, 1907, Feb. 28. J. Ellard Gore.

The Sun-spots of 1906 *December.*

December opened with seven groups of spots on the Sun's disc, all but one, Group 6031, being in the northern hemisphere, as were indeed the majority of groups seen during the month. Five of the six northern groups, Groups 6021, 6024, 6026, 6027, and 6030, were legacies from November, and made up together an irregular undulating procession nearly 40° of solar longitude in length. After the last member of this series had disappeared at the west limb by December 7, no important new groups were observed until December 11, when a very active period commenced, which lasted until the end of the year. In the northern hemisphere a very fine procession, 35° in total length and comprising Groups 6042, 6045, 6048, and 6049, was under observation from December 11 to December 25; in the southern, Group 6046, the second largest of the year, was in view from December 12 to December 24. A third procession in the northern hemisphere, less important than the two already noted, lasted from December 19 to December 31, and included Groups 6051, 6052, 6053, 6057, and 6060; and the year closed shortly after a magnificent stream, Group 6059, had come into view in the northern hemisphere. An extensive but feeble disturbance, including three small groups on December 31, Groups 6058, 6062, and 6063, had also begun to form in the southern hemisphere as the year came to an end.

On the whole, December ranked as the second most disturbed month of the year, being distinctly inferior to July, but slightly more active than March.

Rotation 712 began 1906 December 15·64.

Spot-Groups in 1906 December.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1906.	1906.	days.	1906. d	°	°	
6031	Dec. 1	Dec. 6	6	Dec. 2·9	168	− 8	Sparse stream.
2	1	8	8	7·8	103	+ 7	Small spot.
3	4	5	2	6·2	125	− 9	Very small spot.
4	5	12	8	11·0	61	−12	**Return.** Small spot.
5	7	11	5	5·2	138	+15	Irregular cluster.
6	7	10	4	7·7	105	+13	Small regular spot.
7	7	11	5	11·5	54	−12	**Return.** *f.* Gr. 6034.
8	8	11	4	5·5	134	− 7	Small regular spot.
9	8	9	2	10·2	72	+ 9	A very small spot.
6040	9	9	1	6·3	123	− 4	Small spots *f.* Gr. 6038.
1	9	16	8	10·9	62	+12	Cluster in rapid change.
2	11	22	12	16·8	345	+13	Fine straight stream.
3	12	13	2	8·7	91	+19	Two very small spots.
4	12	13	2	10·9	62	+16	Small spots *n.* Gr. 6041.
5	12	24	13	18·6	321	+14	**Return.** Large regular spot.
6	12	24	13	18·6	321	−17	Very fine composite spot.
7	14	18	5	13·3	31	+ 4	Straggling changing stream.
8	14	22	9	17·9	330	+13	Stream *f.* Gr. 6045.
9	18	25	8	19·4	310	+15	Stream *f.* Gr. 6045.
6050	19	19	1	19·7	306	−12	Small spot *nf.* Gr. 6046.
1	19	29	11	24·7	240	+19	Regular spot.
2	20	25	6	26·4	218	+19	**Return.** *f.* Gr. 6051.
3	22	31	10	25·8	226	+18	Stream *p.* Gr. 6052.
4	25	27	3	29·5	178	−13	Small regular spot.
5	26	27	2	21·5	282	− 7	Pair of small spots.
6	27	30	4	31·9	146	+12	Short irregular stream.
7	27	30	4	27·5	204	+19	Very small spot.
8	29	1907. Jan. 7	10	1907. Jan. 1·9	132	− 7	Straggling stream.
9	29	9	12	3·8	108	+ 7	Magnificent stream.
6060	29	1906. Dec. 31	3	1906. Dec. 28·0	197	+16	One or two small spots.
1	31	1907. Jan. 4	5	29·5	178	+ 8	Short stream.
2	31	7	8	1907. Jan. 1·7	135	−13	Stream *s.* Gr. 6058.
3	31	3	4	2·3	128	−14	Small spots *f.* Gr. 6062.
4	31	2	3	5·6	84	+18	Very small spot.

Notes on the Principal Spot-Groups.

Group 6034.—Return of Group 6004. Small regular spot. Final stage.

Group 6037.—Return of Group 6006. A cluster of very small spots. Final stage.

Group 6042.—A fine straight stream in constant change. It finally coalesced to form one very large composite spot. Area on December 18, about 600.

Group 6045.—Return of Group 6019. A large regular spot, crossed by one or two bright bridges in its earlier days. Area on December 19, about 200.

Group 6046.—A very large composite spot, the second largest of the year. A train of spots formed *f.* the chief spot, and rapidly developed, undergoing many changes. Area on December 19, about 1000.

Group 6048.—A stream forming between Groups 6042 and 6045. It underwent great and frequent changes. Area on December 17, about 300.

Group 6049.—A very small spot *f.* Group 6045, and rapidly developing into a fine stream. Area on December 21, about 300.

Group 6051.—A regular spot. Area on December 24, 150.

Group 6052.—Return of Group 6026. A small regular spot *f.* Group 6051.

Group 6053.—A rapidly changing stream of spots, forming between Groups 6051 and 6052. On December 24 the group was one large, complex, and almost continuous stream of area about 600.

Group 6054.—A magnificent stream chiefly composed of two very large composite spots. Early in 1907 January it was seen to be as large as Group 5933, the largest group of the year 1906.

E. WALTER MAUNDER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

N. L. de La Caille.

GENTLEMEN,—

While we rejoice in the way in which, in our own time, the stars of the southern heavens have been surveyed *, chiefly by the labours of Gould, Struve, and Sir David Gill, it is interesting sometimes to look back upon the past when that hemisphere was almost unknown to astronomers. The first to form a southern catalogue was Halley, from observations at St. Helena in 1677–8, but these were made with a sextant and included only 341 stars. The real annexer, so to speak, of the southern hemisphere for astronomy was La Caille, and a study of his career impresses the reader so strongly in his favour both as a man and as a scientific enthusiast, that perhaps some of your readers may be interested in going over it again.

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* Sir John Herschel's epoch-making work referred principally to the nebulæ, completing his father's in the northern hemisphere.

The material is chiefly furnished by the 'Discours Historique,' which precedes the 'Journal Historique du Voyage fait au Cap de Bonne-Espérance,' published in 1763, the year after the author's death, the 'Discours' being contributed by one of his intimate friends, who does not give his own name, but who is now known to have been the Abbé Carlier.

Nicolas Louis de la Caille was born on the 13th of March, 1713, at Rumigny, on the little river Aube, in the western part of what is now the department of Ardennes. His father, of the same name as himself, had served in the Gendarmes and in the Artillery, and in 1713 was leading a retired life, but pursued his bent for mechanics and invented several ingenious machines. Desiring to improve his position the better to educate his son, in whom he early noticed a taste for knowledge, the Sieur de la Caille constructed a paper-mill and carried it on for some time with success; but in consequence of the bad conduct of some of his workpeople and other misfortunes, he unfortunately was led into expenses which eventually nearly exhausted his resources. He now came under the notice of the Duke of Maine, who proposed to put him at the head of an establishment to be founded in an American island, and La Caille repaired to Nantes for the purpose of embarking. The scheme, however, fell through; but he became a sort of steward to the Duchess of Maine, managing her estate at Anat, near Dreux. The future astronomer was now sent to the College of Mantes-sur-Seine, the principal of which was a friend of his father, and here he attended the classes of humanity and rhetoric until, in 1729, he was transferred to the Lisieux College at Paris, where he pursued his studies with great diligence, mastering most of the classics and acquiring a pure style of Latinity through his assiduous study of the works of Cicero. Like most classical scholars he imbibed also a great admiration of Horace.

After completing the philosophical course at the college of Lisieux, he devoted three years to the study of theology at that of Navarre. But his delight at meeting with the elements of Euclid showed that his true vocation was mathematics. When taking his degree as Master of Arts, a slight put upon him by the Vice-Chancellor (a devoted adherent of the scholastic philosophy) in consequence of an answer which he had given so annoyed him that he determined henceforth to attach himself to mathematical and scientific pursuits. This was about the end of 1736, when he was in the twenty-fourth year of his age. It was then that the Abbé (as he now was) obtained an introduction to Cassini II., who received him with great kindness, and, struck by his talents, said, "Demeurez avec moi, je vous offre ma maison et toute mon amitié." La Caille commenced observing the stars in the month of May 1737, and continued the work with enthusiasm, showing also great skill in astronomical calculations. The younger Maraldi conceived also a great friendship for La Caille, who warmly returned his as well as that of Cassini, whose loss in later times, through a carriage accident, he deeply deplored.

In 1738 Maraldi and La Caille undertook and carried out a survey of the coast of France from Nantes to Bayonne; and in the following year Cassini commissioned his son (Cassini de Thury) and La Caille to re-measure the arc of the meridian from the south to the north of France, which was first done by Cassini I. and the elder Maraldi. The line was to proceed from Perpignan to Dunkirk: soon after La Caille had begun his operations at the former place, an accident nearly put an end to them and to his life. Crossing a swollen stream on horseback, the animal sank, and reappearing without his rider some distance further down the stream, it was thought that La Caille was drowned; but he soon appeared on the opposite bank, wet through, but with perfect sang-froid. Appointed in November 1739 to the Chair of Mathematics at the Mazarin College at Paris, he nevertheless continued to take part in the survey work till it was finished, and had to undergo several hardships in its course. In 1741 he took the office previously held by Fouchy in the Royal Academy of Sciences, contributing on the occasion a valuable paper on spherical trigonometry; also an account of an eclipse of the Moon which he had observed on the 13th of January, 1740, whilst on a mountain near Aix. Before the end of 1741 he published the substance of his lectures under the title 'Leçons élémentaires de Mathématiques,' which was translated into Latin, English, Spanish, and Italian. He observed the comet of 1742 and calculated its orbit. About this time an observatory was constructed for him at the Mazarin College, and thenceforward, says his biographer, "sa vie n'a presqué plus été qu'une seule observation." The results of the survey work were published in 1744; La Caille insisted that his name should not appear on the titlepage, but only that of his colleague Maraldi, who, however, acknowledged his great assistance in the preface. Other mathematical works followed; but we must pass on to the work for which La Caille is principally known. Recognizing the importance of obtaining stellar observations in the southern as well as in the northern hemisphere, it occurred to him that the Cape of Good Hope was the most suitable place to do this, its longitude being nearly the same as that of Western Europe and the air more pure and serene than at most southern places where European nations had settlements. His design was approved of, and the French Government furnished him with the means necessary for carrying it out. Some years before a Bavarian, Peter Kolb, had gone out on a similar commission from Baron Krosigk, of Magdeburg, but had not carried out his instructions, so that the small number of observations made by Halley were practically all hitherto made in the southern hemisphere. La Caille embarked at l'Orient on the 21st of November, 1750; repairs necessary to the ship obliged the voyagers to put in at Rio Janeiro, where La Caille made the best use of the time of delay thus caused: they did not land at the Cape until the 19th of April, 1751. There is no necessity here to go over his work, which is now a classic in astronomy. It was completed in about two years; but before his return to France he received a commission to survey the islands Mauritius and Réunion (then called Ile de France and Bourbon respectively). He remained nine months in the former island, but only about six weeks in the latter; stayed also a few days at Ascension on the return voyage, and finally arrived at l'Orient again on the 4th of June, 1754, and at Paris on the 28th of the same month. Two anecdotes are told us which illustrate his strict ideas of honesty and his scrupulous straightforwardness. Having a special permission to send letters and packages free of duty to France, an offer of a considerable sum of money was made to him for the use of this privilege to convey other things under his name. The answer was that he could not accept it, "ni en qualité d'Ecclésiastique ni en qualité d'honnête homme." The other is that having been allowed the sum of 10,000 francs for his expenses and those of a workman whom he took with him, he kept an exact account, and finding the whole amounted to 9145 francs, insisted on returning the balance to the treasury. He did not neglect, when in South Africa, to make observations on natural history and ethnology, but it would be out of place to describe them here. The calculation of eclipses for 'L'Art de vérifier les Dates' was his work, and of course one of great labour. It is well known that the reduction of his great southern catalogue was one of the many works undertaken by Baily, which, unfortunately, in this case, he did not live to complete. La Caille's other contributions to astronomy were too numerous even to mention here, though we must not quite omit his 'Astronomiæ Fundamenta,' which appeared in 1757, and of which Lalande speaks as "ce recueil précieux d'observations," and the fact, especially interesting at the present time whilst Mr. Cowell and Mr. Crommelin are at work on the same subject, that one of his latest studies was on the theory of the motions of Halley's comet, on the occasion of its appearance in 1759. He calculated also the orbits of two comets which were observed early in the year 1760, though the perihelion passages of both took place in the previous year. Indeed this enthusiastic astronomer worked almost to the end; he died in March 1762, having just completed his 49th year, and left his manuscripts to the care of his life-long friend Maraldi.

Yours faithfully,

Blackheath, 1907, March 6. W. T. LYNN.

The Lyrids, 1907.

GENTLEMEN,—

The Moon will be at the first quarter at the epoch of the Lyrid shower this year and will not seriously interfere with successful observation.

It is hoped that observers will, if possible, obtain places for the radiant on several nights, so that the question of displacement can be more fully tested. In recent years a few very active showers in the southern hemisphere have been observed at the April epoch as under:—

189°	−31°	218°	− 5°
203	−10	226	− 5
218	−31	236	−14

It would be interesting to have further information concerning these streams.

In view of the approaching return of Halley's Comet, its possibly associated shower of γ Aquarids, visible before sunrise at end of April and during the first week in May, is expected to develop unusual strength in the next few years, when its returns will merit careful observation. Yours faithfully,

W. F. DENNING.

Bishopston, Bristol,
1907, March 18.

Problems of Solar Physics.

GENTLEMEN,—

In the article under the above title which appeared in the *Observatory* for February last occurs the phrase, in reference to the discussion on standards of wave-length, "Hartmann also would patch up Rowland's Tables." The sentence does not, in its necessary brevity, adequately express Prof. Hartmann's position. His stand-point is that one wave-length should be adopted, namely, that of the red line "of the cadmium spark in a vacuum measured in air at +20° C. and 760 mm. pressure . . . as $\lambda = 6438{\cdot}6911$," as the invariable standard for all time (*Astrophysical Journal*, vol. xx. no. 1, July 1904). The choice of such a standard would define a wave-length system which would as nearly as possible fall in with Rowland's, and hence obviate the burden of changing from the system of Rowland to that of Michelsen. Unless we are to suppose that spectroscopists have reached finality in the accuracy with which absolute wave-lengths of lines can be determined by interference methods, "we shall be sooner or later compelled to stop at some system of wave-lengths which is relatively, but not absolutely, correct" (*loc. cit.* p. 46). Such a system, according to Prof. Hartmann, is that of Rowland's Preliminary Table of Solar Spectrum Wave-Lengths when corrected as explained in the paper "A Revision of Rowland's System of Wave-Lengths" (*Astrophysical Journal*, vol. xviii. no. 3, October 1903). Yours faithfully,

A. L. CORTIE.

Stonyhurst,
1907, March 17.

NOTES.

Comet Notes.—A new comet, *a* 1907, of the 11th magnitude, was discovered by M. Giacobini at Nice on March 9, in R.A. $7^h\ 4^m\ 31^s$, S. Dec. 18° 21′. The discoverer gives the following orbit from observations on March 9, 11, 13. The elements somewhat resemble those of Zona's Comet (1890 IV.), which are given for comparison:—

			Zona.
T	1907 Mar. 15·66 Paris M.T.		1890 Aug. 7.
ω	315° 10′	1907·0.	331° 22′
☊	96 49		85 22
i	141 54		154 19
log *a* ..	0·3121		0·3114

Identity is not possible, but it seems not improbable that the two comets had a common origin.

Ephemeris for Paris Midnight.

Date.	R.A. h m s	N. Dec. ° ′	Brightness.
April 4........	6 19 27	1 8	0·53
8........	6 16 37	3 13	
12........	6 14 25	5 7	0·40
16........	6 12 39	6 54	

Comet 1905 IV. has been photographed by Kopff at Heidelberg, 1907 March $21^d\ 14^h\ 45^m{\cdot}8$ local M.T., R.A. $14^h\ 58^m\ 36^s$, S. Dec. 21° 18′, Magnitude 13·8. The observations of this comet cover a period of 2 years 2 months.

The Tabular Place was $14^h\ 58^m\ 44^s$, S. 21° 19′, by Dr. E. Weiss's ephemeris. A. C. D. C.

Minor Planet Notes.—N. Liapin, who was the first to photograph the planet WD, gives the following circular elements for it in *Astr. Nach.* 4157:—

Epoch	1906 Oct. 26·5 Berlin M.T.
u	195° 49′
☊	203 7
i	48 8
μ	387″
log *a*	0·6595

The high inclination is noteworthy.

Planet WO proves to be identical with 260 Huberta. VL is probably identical with OE, discovered in 1904.

Planet 452, discovered by Prof. Keeler in 1899, has been named Hamiltonia.

The following discoveries have been made:—

	Date.	Magnitude.	Discoverer.
XM	1907 Feb. 10	13·0	Kopff, Heidelberg.
XN	11	13·4	,, ,,
XO	11	11·8	,, ,,
XP	8	13·0	Metcalf, Taunton, Mass.
XQ	8	14·0	,, ,, ,,
XR	4	15·0	Lowell, Flagstaff, Ariz.
XS	Mar. 4	13·3	Kopff, Heidelberg.
XT	7	12·6	,, ,,
XU	7	13·0	,, ,,
XV	7	13·7	,, ,,
XW	7	13·2	,, ,,

Prof. Barnard obtained an observation of 588 TG on Feb. 12. The R.A. was 51^s less than Bidschof's ephemeris, and the Dec. 6′·7 north of it, but accurate places of the comparison stars are not yet available. *Ast. Nach.* 4163 contains a continuation of the ephemeris till June 19 next, when the planet will be in R.A. $11^h\ 21^m$, S. Dec. 4° 16′, Mag. 14·7. A. C. D. C.

THE ROYAL ASTRONOMICAL SOCIETY OF CANADA.—This Society, whose annual volume we have received and noticed in these pages with pleasure for some years past, is making new arrangements of its publications. It is now proposed to publish a Journal every two months (possibly it will later on appear every month), to take the place of the 'Transactions' hitherto published.

The headquarters of the Society is in Toronto, and from no. 1 of volume i. of the new Journal we learn that there is a Section at Ottawa, where the lately established Dominion Observatory, of which Dr. W. F. King is Director, is located. The matter in this number of the Journal consists largely of Reports and accounts of Meetings, of more interest to Canadians than to others. There are several papers and notes on magnetic subjects by Dr. Stupart, and from one of these, which gives a tabulation of magnetic disturbance and aurora, the following paragraph may be quoted:—

"It would then appear that when large magnetic disturbances occur over the whole continent, auroræ which never occur without magnetic disturbance may yet be absent in many localities."

THE PRINTING CHRONOGRAPH: A CORRECTION.—We regret, when writing the note on this subject on p. 145 of our last number, to have overlooked the printing chronograph in use at the Durham Observatory, described by Prof. Sampson in the 'Monthly Notices' for April 1903. This instrument, made by a Chicago firm, was designed by Prof. G. W. Hough, of the Dearborn Observatory, Illinois, more than twenty years ago.

ERRATUM.—March number, p. 136, line 11 from bottom, *for* "with" *read* "without."

PROF. BELOPOLSKY, Mr. E. W. MAUNDER, and Prof. KAYSER have been elected Honorary Foreign Associates of the Italian Spectroscopic Society.

THE next Meeting of the Royal Astronomical Society will be on Friday, April 12; of the British Astronomical Association on Wednesday, April 24.

FROM AN OXFORD NOTE-BOOK.

MANY friends will be interested to learn that Mr. and Mrs. Evershed have arrived at Kodaikanal and settled down to work with enthusiasm. They visited the Harvard, Yerkes, Mount Wilson, and Lick Observatories, and though they had very bad luck at the second and fourth in the way of weather, they were more fortunate in their month on Mount Wilson. They sailed from San Francisco on November 30, and travelled *viâ* Japan, Manila, Singapore, and Colombo to Madras. On January 14 they came in for a partial phase of the total eclipse, mid-eclipse being at noon, and the ship's officers lent Mr. Evershed three sextants for making observations.

At the date of writing (February 18) they were established temporarily (though in a house of their own) pending the completion of a dwelling specially being built for them. They were having fine dry weather and good definition in the early morning, and an independent letter received from the Director indicates as his chief trouble at present an anxiety lest Mr. Evershed should work too hard with the spectroheliograph.

AN Italian newspaper of February 3 last gives some interesting news concerning a projected observatory. It has been proposed by the University of Bologna, and agreed to by the city, that the historical Villa Aldini should be used as the site of a new observatory, instead of the ancient *Specola* of the Via Zamboni. The dilapidated condition of the old building, and its situation in the midst of the town, made it impossible either to set up new instruments or to carry out observations under proper conditions. The idea has been carefully considered by the Municipality, and it has been decided that the Commune shall contribute 100,000 lire towards the new Astronomical Observatory on condition that the Government contributes a like sum towards the necessary expenses. In addition the Commune places the Villa Aldini and its adjoining grounds at the disposition of the University.

IT is often a difficult matter to decide whether a book containing some astronomy among much that is not astronomy, though full of interest, shall be added to our R. A. S. Library. An excellent instance of a difficult case is 'Leonardo de Vinci's Note-Books,' by Edward McCurdy (Duckworth & Co., 1906: New York, Charles Scribner's Sons). The astronomical part is only a few pages (pp. 85–92), but the note-book of an artist and a thinker who began in 1508 to make a "collection without order, made up of many sheets which I have copied here, hoping afterwards to arrange them," has something on almost every page worthy of our attention, even though it be the dire prophecy at the end that "whereas at first young maidens could not be protected... either by the watchfulness of parents or by the strength of walls, there will come a time when it will be necessary for the fathers and relatives of those maidens to pay a great price to whoever is willing to marry them, even though they may be rich and noble and exceedingly beautiful. Herein it seems certain that nature desires to exterminate the human race, as a thing useless to the world and the destroyer of all created things." One wonders what personal experience of the writer suggested this dreadful note? Habits of careful observation are indicated in such passages as this, from the notes on the possibility of artificial flight:—

> Man is also possessed of a greater amount of strength in his legs than is required by his weight. And in order to show the truth of this, place a man to stand on the sea-shore, and observe how far the marks of his feet sink in; and then set another man on his back, and you will see how much deeper the marks of his feet will be. Then take away the man from his back, and set him to jump straight up as high as he can; you will then find that the marks of his feet make a deeper impression where he has jumped than where he has had the man on his back. This affords us a double proof that man is possessed of more than twice the amount of strength that is required to enable him to support himself.

This was apparently before the days of professional strong men, who could have given even more striking illustrations of the point. Leonardo's "explanation of why the Sun seems larger in the west" is a little puzzling. After remarking that "certain mathematicians contend that the Sun grows larger when it is setting, because the eye sees it continually through atmosphere of greater density," he gives as his own "real reason why the Sun grows larger" that "every luminous body appears larger, as it is further away" (p. 87). Coming from one who wrote carefully about perspective and said, for instance,

> I find by experience that if the second object is as far distant from the first as is the first from your eye, although as between themselves they may be of equal size, the second will seem half as small again as the first" (p. 213),

the statement about the Sun must have some rather obscure meaning, for Leonardo apparently realized that, owing to the effect of parallax, bodies are "three thousand five hundred miles" further away when setting, and he gives a diagram to show this. He gives a good illustration of specular reflection, and remarks that the Moon's surface cannot be polished, since it shows no such phenomenon, and attempts an explanation of lunar halos. Some of his aphorisms take an unusual turn, as:—"No counsel is more trustworthy than that which is given upon ships that are in peril."

FROM 'The Secret of the Moon,' by Morice Gerard (London: George Newnes, n.d.):—

CHAPTER XIV.

Dr. Morpeth was driving home about nine o'clock from "Forelands" on the night that little Alice's illness first declared itself. He had paid his last visit to the child for the day. . . . . It was now dark. There would be a new Moon later, but it had not yet risen, although there were faint indications of its coming at the edge of the sky.

THE following anonymous letter was received with no particulars regarding its origin beyond the date 22/2/07 :—

Informally obtruding is excused, I am sure, after you read that my object is strictly in the interests of science.

The four type-written statements are based on the fact that *matter* is a product of ether.

The claim evanishes the theory of "attractive gravity," and accounts rationally for all the phenomena of planetary motion.

Perhaps you will give the subject your attention, and that is all this asks.

1. All bodies in space in virtue of their mass and temperature have a repelling force against the cold outside ether.
2. This force is returned upon each mass by the ether, and gives to each its gravitative ratio in space, and is an intelligible explanation of *gravitation and its origin.*
3. The repelling force of each mass results in grading the density of the ether from tenuous at centre to dense at circumference.
4. Reduction of temperature in each mass assures reduction of space occupied by each, with corresponding decline of gravitative force until *equilibrium is restored.*

THERE cannot be many places in the world wherein the signs of spring are more easily noted than in Oxford. In the first place, it shares with the rest of the Temperate Zone the changes on the face of Nature which have inspired so many poets. Secondly, there is a tumult of young vigour which orders itself into different athletic games with different seasons. Finally, there is the well-marked end of the Lent term, which all Oxford recognizes as meaning that between now and October there is only the trifling Summer term actually claiming work-a-day thoughts. We can work in other months if we choose, but it will be because we choose, and not because we must. And the influence of this genial thought is wonderful; it extends to the very games, which about this time of year seem to be played for sheer pleasure and not as a business. To give an example: I was attracted a few days ago by the sight of what appeared to be two games of football on the same ground. They turned out to belong really to the same match, which had been arranged among the boating men of a certain College. They had been slaves to their racing-boats till now, but under the inspiring influence aforesaid were playing football for fun. They had appointed two referees, one a football expert with the regular whistle and the other a boating expert with a horn such as is used to cheer on a boat to its bump. The referees occasionally differed in opinion, so that when I became a spectator one was superintending half the players in trying to kick a goal, and the other was encouraging a scrimmage (without any ball) in a different part of the ground. After the (hopelessly unsuccessful) try, the others came to reinforce the scrimmage and brought the ball with them; and the putting of it into the scrimmage was a solemn event prefaced by the measured counting "five, four, three, two, one, go!" which usually marks the starting of bumping races. But the ball did not seem to be a very important part of the game at any time. On another occasion, when it had gone astray, three gallant young forwards, with arms interlocked and heads down, went right through their opponents, and ran, united, the whole length of the ground to the opposite goal-line, when they threw and touched each other down. It was a most exhilarating spectacle: the perfect seriousness and absolute joyousness of the fun was something to make the heart young again. The almanac merely says "March 21. Sun enters the sign Aries. Spring commences, 6^h 33^m *Aft.*"

BUT the above prose reference to the season will perhaps scarcely be accepted as an equivalent for some poetry; and it is some time since we had any. I would venture to call attention to a book called 'The Human Epic,' by John Frederick Rowbotham (London: Gay & Bird, 22 Bedford Street, Strand: price 2/6 net). It is in 40 cantos of about 30 stanzas, each of 9 lines—say about 10,000 lines, a good half-crown's worth. The ridiculous pretensions of the author, who styles it the "twelfth epic poem of the world," and himself "the Homer of modern times," make one take up the book merely to laugh; and there are undeniable opportunities afforded, as in the following reference to a rainy period in our history (p. 14):—

Thus fell the rain ten thousand thousand years.
In sheets, in showers, in deluges it fell.
And could the eye of man, oppressed by fears,
Have viewed the rain's interminable swell,
Thus would the tongue of man its terrors tell:
"Will this eternal downpour never cease?
"Will ne'er the clouds their wild convulsions quell?
"Each hour the torrents seem but to increase;
"Nor any lull portends tranquillity and peace."

The author seems to find it difficult to get on without someone soliloquizing, and is driven to satisfy this need even by the introduction of the man-in-the-street to contemplate a really majestic deluge. He is more successful when he personifies the primeval nebula (p. 3):—

Then thought the Mist: Lo here I stand and seethe!
Lo here have I for million million years
So stood, so seethed! My misty rings I wreathe;
Nor any end to my repose appears.
When will an end arrive, that never nears?
What, what is end? What may beginning be?
On neither verge my apprehension clears.
Eternity alone is known to me:
To rest unalterable everlastingly.

It sounds to an unskilful ear as though there is something dangerously near good poetry here: and there is better than this in places. A good many of the ten thousand lines have still to be explored; meanwhile this passing reference may attract the attention of someone better able to assess the merits of a rather *weird performance.*

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. MAY, 1907. No. 383.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1907 April 12.

Mr. H. F. NEWALL, M.A., F.R.S., *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder. Eighty-eight presents have been received since the date of the last Meeting, among them being three publications from the Besançon Observatory, and enlarged reproductions of Astrographic-Chart Plates in continuation of series already received—20 from the Royal Observatory, Greenwich; and 47 from the Paris, Algiers, and Toulouse Observatories, presented by the French Minister of Public Instruction.

The Astronomer Royal. The discussion of the photographs of Eros, taken at Greenwich to determine the Solar Parallax, is now complete. Fellows may remember that an account was given at the January Meeting of a determination of the division-errors of the réseau which was needed to enable us to get the greatest accuracy obtainable from these results, and I am now able to bring before the Society the value deduced from the Greenwich photographs. It may be remembered that the plan of the Eros Commission was that the photographs at a number of co-operating Observatories should be all discussed together, and a combined result obtained. That is a matter of some difficulty, and obviously to discuss adequately such a mass of heterogeneous material must take a great deal of time. I would here express my satisfaction that Mr. Hinks has faced that task, but I think it is desirable that meanwhile each Observatory should work up its results, and deduce the value of the parallax from the material available there. It will be understood, of course, that there will be much more material available by combining the photographs taken at a number of observatories, but at the same time there is difficulty in bringing them all into one system; and there may be systematic differences which may be shown up to some extent by discussing the results separately. At this opposition of Eros photographs were taken from Oct. 1, 1900, to Feb. 2, 1901; but only during part of that period, that is from Oct. 14 to Jan. 18, was the material suitable for determination of parallax. The photographs obtained before and after are available for getting errors of the ephemeris of Eros, but the parallax factor is not sufficiently large to make it advisable to use them for the parallax, as will be seen from this diagram, which Mr. Dyson prepared, showing the range of hour-angle at which photographs could be taken at different dates and the consequent maximum difference of place due to parallax. The first thing to be done was the determination of the motion of Eros, and this, which forms the essential basis of the determination of the parallax, was skilfully done by Mr. Eddington. He discussed all the photographs available and grouped them for periods of five days or less, and so deduced the normal errors of the ephemeris of Eros as given by the two instruments, the Astrographic telescope and the Thompson; the mean of the two being finally taken, a curve was drawn which gave the daily motion of Eros very accurately. It was found there was considerable change of the error of the ephemeris from day to day, which amounted at times to as much as 0″·10 a day, but as that could be determined very accurately by this method I think there is really no uncertainty arising from this cause.

Passing on to the parallax results, I show here a table giving the corrections to the solar parallax 8″·80 deduced from the measured right ascensions from October 14 to January 18, the number of plates compared in each case, and the difference of parallax between evening and morning, which ranged between 10″ and 28″ and was the best we could get. It will be seen that the mean correction to this adopted value of the solar parallax is +0″·005, with a probable error of ±·0045, and I think that probable error may be fairly taken as representing the accuracy of the measurement. I should add that this table has been formed by a combination of the Astrographic and Thompson photographs. Values of the parallax were first deduced separately from each instrument, and then finding that they could practically be reduced to the same system the two series of plates were combined, so that naturally the combination gives greater weight than when each series was treated separately.

The next diagram gives the discussion of declinations. Of course the geometrical conditions are not favourable for this method, and it will be seen that the weights are very small in most cases. I should explain that the weights are formed on the same system as for the right ascensions, and weight 1 corresponds to a pair of plates with a parallax difference of 25″. It will be seen from the amount of this probable error* that the result is not deserving of anything like the same confidence as the result from right ascensions, still it is interesting to give it and it is important in other ways.

Finally, I show a tabular statement of the various results that have been obtained. As I have before explained, the results were first worked out with provisional division-errors of the *réseau*, and then further corrections were applied and these results were obtained after correction for the residual division-errors. The astrographic plates gave a parallax of 8″·793 when the reference stars were used, and 8″·809 when comparison stars were taken as fundamental points. The first result has been put in brackets, because I think it is fairly obvious that the reference stars being much brighter than Eros, and also further from the centre than the comparison stars, may introduce some systematic error. From the Thompson plates, using the comparison stars, the result for parallax was 8″·800 ±0″·0063.

The combined discussion of the Astrographic and the Thompson plates, using the comparison stars only, gives the value for the solar parallax 8″·805 ± 0″·0045, which I have said is the best result we can obtain out of the photographs we have taken. Just to test the different results that could be got out Mr. Eddington combined the photographs taken from evening to morning, which give 8″·807 with a probable error of ±0″·0036, and then, again, those from morning to evening, which give 8″·801, with a probable error of ±0″·0080. At first sight the significance of that is not evident, but the real point is that the same stars were used for evening and morning photographs, and another set of stars on the following night, so that the morning to evening comparison introduces the accidental errors of reference stars, which is shown up very markedly in the probable errors. Then in order to show what the effect of these corrections to the division-errors has been, I have given the results which were obtained in the first instance, the provisional results before this further investigation of division-errors, and it will be seen that, as in the other case, the reference stars give a smaller value than the comparison stars in the case of both instruments.

We discussed some photographs with the telescope in one position only, although the parallactic factor was then small as the telescope is reversed on the meridian. These results are not very satisfactory, and they do not seem to help very much; there is, of course, a large probable error. In the case of the declinations the division-errors do not affect the results in the same systematic way as for right ascensions, and so these have not been corrected for residual division-errors. In fact those errors have not been determined, as it seemed to be sufficient to take the division-errors as first determined.

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* The correction to solar parallax 8″·80 was +0″·019, with probable error ±0″·014.

The correction for residual division-error in the case of the Thompson is practically negligible, and there are three reasons for that. In the first place, the scale of the Thompson is double that of the Astrographic, and so the division-errors are halved in their effect on the result. And another point was that Eros was not kept so exactly to the centre of the field, and since its position varied considerably in different photographs, the effect of division-errors was rather accidental than systematic. Further, the Thompson telescope was, generally speaking, not reversed on passing the pier, so that the réseau was not reversed and the division-error did not affect the result.

Sir David Gill. I should like to ask whether the correction to the computed orbit varied according to the position of the Moon with respect to the planet? I think that is a subject which might be discussed with great advantage, and from Eros we should have a powerful determination of the Moon's mass. It is naturally in part a cause of the correction that has to be allowed for in discussing the motion of the planet. Next I am rather surprised to find that a sensible factor can be obtained from observations in declination at a single observatory. It must have been only in the extreme case when the planet was photographed near the zenith and then very far from it; for, of course, as everybody knows, the parallax factors in declination are symmetrical with respect to the meridian; but as the weights attached to these results are small, little importance can be attached to a value of the parallax derived from these observations, although, of course, if the parallax effect does vary sufficiently it may give some small weight. I congratulate the Astronomer Royal in having the results of the solar parallax so very near to the one I myself arrived at; and when I said so to him privately a little while ago he was kind enough to say that I had made a very good guess. (Laughter).

Mr. Eddington. The Astronomer Royal has dealt with almost everything, so that I can think of little to add except, perhaps, that the action in rejecting the results from reference stars may call for some comment, seeing there was considerable difference between them and the others. As regards probable error, determinations from the reference stars and from the comparison stars are not independent; that is to say, if by bad luck we got too high results from comparison stars we should get, as far as accidental error is concerned, too high a result from the reference stars, and it would be no use taking the mean, for we could not reduce the error in that way. A decided shift is shown between the reference stars and comparison stars when the plates low down on one side of the meridian are compared with those on the meridian.

Sir David Gill. That is to say, there is a spurious parallax.

Mr. Eddington. Yes. An apparent parallax between a plate low down and a plate near the meridian, taking them on the same side of the meridian so as to avoid difficulty about possible undetermined division-error. It is not clear whether that is due to their being fainter than the reference stars, or due to their being nearer to the centre of the field. Unfortunately there is not sufficient evidence to decide that. In any case, I think, as the Astronomer Royal pointed out, it seems permissible to assume that Eros will behave more like the comparison stars than the reference stars; in fact, if anything, the value given by the reference stars would be on the wrong side; we are not erring, because the comparison stars were, if anything, a little brighter than Eros, and not quite so near to the centre of the field, so that one would expect the true parallax would not be between the results from reference and comparison stars, but a little beyond the result from comparison stars.

Prof. Dyson. I should like to be allowed to express my pleasure that this discussion has been finished and to congratulate the Astronomer Royal upon having got such good results from such a heavy piece of work. It seems that a number of most interesting points have come out in the discussion. One knows that apart from the solar parallax there is a great deal to be learnt as to the way of treating photographs and other things, as is illustrated by the question about the distribution of bright reference stars over the plate as compared with the faint stars near the centre. We have also learned a good deal about the effects due to printing of *réseau* lines, and on other points we have gained much information.

Prof. Turner. I think, Sir we were all interested in knowing on which side of 8″·800 the determinations were coming out. Mr. Hinks got a value* a little less, I think, and the Astronomer Royal has now produced results which are a little greater. It is well to remember what an enormous amount of work is involved in getting this result—six years' labour; and I would add how very easily it might have been different. By way of illustration, I would like to remind you in that long table given by the Astronomer Royal there was at the bottom a large residual—I think, if my memory serves me, it was 0″·277, and if that one residual were rejected we should get results less by 0″·004, very nearly 8″·800. I do not mean for a moment to suggest, although the residual is much greater than any other, that it ought to be rejected, but the fact remains that if it *had happened* to be cloudy on January 17–19 the final result would have come very close indeed to 8″·800. (Laughter.)†

Mr. Hinks. Naturally, Sir, I have been profoundly interested in this weighty contribution to the general problem made by the Astronomer Royal this evening. And it was not in the least a matter of surprise to me to find the Greenwich results give such an exceedingly small probable error. For the Astronomer Royal very kindly gave me an advance copy of the Greenwich photographic star-places determined in the course of this work for use in my discussion of a standard system of comparison-star places from a large number of photographic series. All kinds of troubles have arisen from some of these, but Greenwich has provided one of the two or three series that have given no suspicion of trouble of any kind, so that I was very much prepared to find such splendid results for the solar parallax come out.

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* [Mr. Hinks's result from 300 exposures was $\pi = 8''\cdot797 \pm 0''\cdot0047$ (M. N. lxiv. p. 725). This was an interim result from only a small part of the whole material under discussion.—Eds.]

† [On further examination it has been decided to reject this large residual, as the photographs on which it depends are not satisfactory, and the final result for the parallax comes out 8″·800 ±0″·0044.—W. H. M. C.]

If I may make one or two remarks upon points of detail, I was much interested in the method of determining the tabular error of the ephemeris first, and solving for the parallax afterwards—a different process, I think, from what has been used before. If I understand aright, there was no unknown but the parallax left in the equations of condition, so that no least-square solutions were required. [*The Astronomer Royal.* None.] That is very comfortable. (Laughter.) Mr. Eddington's results for the difference of parallax derived from the bright reference stars and the fainter comparison stars are very valuable, and they confirm the opinion that seems to be growing on all sides, that one cannot usefully employ in parallax determinations stars distributed over the whole plate. Probably one ought to have provided from the first for comparison stars in quite small fields about the planet, and it seems to me that we shall have to do so in the end. With regard to declinations and the small weight one gets from them, it would be very interesting to have the opinion of the Astronomer Royal whether it will be worth while, in a general solution, to discuss declinations at all—whether we should attempt the heroic and double the work for a small increase of weight. Advice on this point would be useful.

Finally, I cannot help thinking that an advantage they have had at Greenwich, which has conduced to these good results, lies in the simple processes of reduction employed—processes which we owe in the first place to Prof. Turner. It seems to me that the people who go in for that simple rectangular coordinates process of reduction have a tremendous advantage over others who complicate their work by computing corrections unnecessarily.

The Astronomer Royal. I think Mr. Eddington could best answer Sir David Gill's question as to the lunar inequality. That was a matter of discussion, but I did not refer to it in this paper because it is another story and really does not affect the solar parallax. At one time there seemed to be some evidence of sinuosity in the curve, but I do not think it comes out clearly enough, and it seemed to be contradicted afterwards, and nothing could be deduced with certainty. The lunar influence occurs in the first table, but it is not supported by the other two; the other two contradicted it.

Mr. Hinks. Did you discuss the effect of variation of magnitude?

The Astronomer Royal. Yes; that was examined, but we could not find anything from it, as the results were grouped for periods of approximately 3 to 5 days, the irregular groups depending on the material available.

The other point Sir David Gill raised about parallax in declination I am not prepared now to go into very deeply; but, as Prof. Newcomb explained in a paper some time ago, it is possible to get a value of the parallax from plates taken east and west combined with those near the meridian, but, as a matter of fact, the parallax factor is small. As Mr. Hinks has asked my advice about dealing with declinations, I should say leave them alone; but I thought it was worth while, as we had gone so far, to put down the result, although the size of the probable error that is given with it shows how much it is worth. At the same time they are not altogether to be despised, because if you compare the probable error resulting from these declinations with the probable errors in some of the determinations of parallax given by Newcomb in his Table, you will find they are much smaller; but of course the result is not comparable with the result which can be got from right ascension. The idea in regard to declination was to combine north and south observatories, but unfortunately circumstances did not allow of that. Quite apart from the value of solar parallax, however, it is interesting in showing whether there is systematic error between right ascension and declination, and the agreement of the two is so far satisfactory; it is more from that point of view that this was discussed. With regard to the amount of uncertainty, I agree with Prof. Turner. We are pleased to find we have a result here so near Sir David Gill's, but at the same time I must point out that the probable error is $\pm 0''\cdot 0045$, and we might have had $8''\cdot 80$ or $8''\cdot 81$ for the parallax.

The President. This short paper which the Astronomer Royal has communicated to us represents an enormous amount of labour, and I am sure he must be pleased and Mr. Eddington must be pleased to have completed the work with such satisfactory results. I understand Father Cortie has some photographs which I should like to call upon him to show us.

Father Cortie. It will be remembered that at the January Meeting a photograph was shown of *o* Ceti, taken by Father Sidgreaves on December 1, 1906, comparing the spectrum of this variable at the last maximum with that of the 1897 maximum, the line $H\beta$, which was absent in 1897, being prominent in 1906. The photograph on the screen is to illustrate the sensitiveness to the red band of the spectrum of plates dyed with pinachrom. In the centre strip of the photograph taken on December 1, the sensitiveness of the Edwards's Isochromatic plate terminates just before the 3rd band of Dunér's system at λ 5855. The same plate stained with pinachrom and exposed when wet on the night of December 9th, shows the 1st, 2nd, and 3rd bands of Dunér, and extends to λ 6600 approximately. The third photograph was taken on December 13 on a Mawson & Swan "Felixi" plate also stained with pinachrom and exposed wet. On this night no impression at all could be obtained of the red end of the spectrum on the evidently slower plates, Wratten and Wainwright's Panchromatic, and Edwards's Isochromatic stained, with cinachrom. The red end on the "Felixi" dyed with pinachrom has given a most intense impression on the plate, and I am inclined to think that the line at the limit of the spectrum photographed is the red line of hydrogen. The next slide shows the origin of these prominent bands in the spectrum of *o* Ceti, and in other stars of Secchi's 3rd type, as due to titanium flutings. The slide was prepared by Prof. Fowler to illustrate his paper on this subject in the 'Proceedings of the Royal Society,' vol. lxxiii. (1904). Prof. Fowler tells me that the compound of titanium which gives these flutings is most probably the oxide. The third slide shows a series of spectra of stars photographed by Father Sidgreaves, demonstrating the gradual replacing of the flutings by lines, until, in the process of transition, a spectrum like that of the Sun is at last obtained. In the current number of the *Astrophysical Journal* is a paper by Prof. Hale and Mr. Adams which demonstrates the existence of titanium flutings in the spectrum of sun-spots. Hence we have a further bond of union between the stars of Secchi's Type III. and the Sun by the existence in them and in the spots of the Sun of the flutings due to titanium oxide. Runge and Paschen have shown the existence in the Sun of the line spectrum of oxygen. The evidence for the presence of oxygen in the Sun is thus strengthened by this connection between the stars of Type III. and the sun-spots. It also lends weight to the physical side of the speculation recently advanced by Prof. Turner as to the variability of the stars of this type being due to their spottedness, viewed in their different aspects from the Earth, since it is evidence as to the probable existence of spots in stars of this type.

Prof. Fowler. I may say I have made many experiments relating to the origin of the bands of such stars as Mira, which have led me to the view that they are due to oxide of titanium, and I am now occupied in collecting data in the hope of convincing others. It seems to me that the relative variation of sensitiveness Father Cortie has referred to is important. I should like to congratulate him on the work he has done in this direction.

Sir David Gill. It will be remembered that there has been going on—and still is—a controversy between my friend Mr. Hinks and Dr. Cohn on the question whether meridian observations made with the movable-wire recording micrometer are free from personality depending upon magnitude. The paper I present to the Society has some bearing on the subject of this controversy.

[Sir David Gill proceeded to explain the investigation. Groups of three stars of approximately the same declination and separated by a few minutes of right ascension were chosen. The first and third stars were of the seventh or eighth magnitude, the middle one being a bright star. The distances between the bright stars and each of the two adjacent stars in its group were measured with the heliometer, and the differences of R.A. determined. The differences of R.A. were also found by direct measurement (1) with the 8-inch transit-circle and recorded on the chronograph in the usual way, (2) with the new reversible transit-circle, the recording micrometer being used. Comparison of the differences of R.A. found by the heliometer with those found by transits showed that in (1) there was a magnitude personal equation amounting to $-0^{s}{\cdot}014$ for one observer and $-0^{s}{\cdot}068$ for another for ninth magnitude stars as compared with 4th magnitude. In (2) the amount was in general smaller, but in one case was −0·029 for ninth magnitude stars.]

I think there is a great deal to be said in favour of the freedom of the movable-wire method from personality dependent on magnitude, but it will be easy to follow all this in the tables attached to the paper which you will see in the copy of the 'Monthly Notices.'

Prof. H. H. Turner. I had the advantage of receiving a copy of this paper in proof, which I read with the greatest interest. So far as Sir David Gill's conclusion goes, that this method of observation is *very likely* to be comparatively free from personal equation, I am entirely with him. But if he means to urge further that it is *always* free from personal equation, I should hesitate. It seems to me that if we have only one single case (and there is such a case indicated in the paper) of personal equation, it leaves us with the necessity of determining the personal equation in all cases; we shall hope to find that it is small, but we cannot trust a catalogue made in the belief that the method gives freedom from personal equation. And I would go further even. An observer at one time may be different from himself at another time. An observer at one time is practically another observer at another time; so while accepting this very welcome evidence that the method is much more likely to be free from personal equation, I should hesitate to say that the evidence is as yet sufficient to claim immunity.

The second remark I would like to make is that to a large extent Sir David Gill's work does not seem to me to cover the exact point tackled by Mr. Hinks. Mr. Hinks, if I understand him rightly, has been determining magnitude equation, not between magnitudes 3 and 8, but between 8 and 9½, somewhere near the end of the scale, and my own experience in determining magnitude equation (by comparison of the Cambridge observations with photographic plates) gave a curve which, for the bright stars 1 to 3, was very nearly a straight line: and then there was a tremendous falling off from 8·0 to 11·0. Well now, of course, if you connect stars 3 to 8, or even 3 to 9, you may get a line with small slope, but if you investigate the end of the curve you might get a big one. Possibly there may be some later opportunity for making experiments to see what sort of magnitude equation there is in faint stars.

Mr. Hinks. I should like to express gratification that such excellent results have followed upon the unfortunate difference of opinion that has arisen between Dr. Cohn and myself. Perhaps I may refer to that question for a moment. Dr. Cohn's catalogue of fundamental stars was observed at Königsberg with the moving wire, and seemed to be not quite homogeneous. You remember that the planet Eros went up in a great loop and came down again. The stars around the top of the loop, in the higher declinations, seemed to be affected by magnitude equation when compared with the mean of eight photographic series ; those lower down did not: and it was this want of homogeneity that made me come with great regret to the conclusion that perhaps Dr. Cohn's fundamental system was not precisely adapted for the discussion of the solar parallax. I should hesitate to consider this result in the light of a destructive criticism on the beautiful method of the moving wire, which he was the first to use. In fact, the discussion of the photographic places of the stars of lower declination makes it quite evident that he has there, by using the moving wire, very nearly eliminated the magnitude equation altogether. There is just the trouble that, in part of the work, considerable magnitude equation seems to have been left, and that was what caused the difference of opinion, because, naturally, he thought that the error was in the eight series of photographs, and naturally I thought it was in the visual observations.

The results given by Sir David Gill are of extreme interest, and I cannot help wishing that he had had an 18-inch heliometer instead of a 7-inch, because the latter will deal with stars only down to the 9th magnitude, and, as Prof. Turner points out, the really exciting part of the curve comes between magnitudes 8·5 and 10. I had the great pleasure eighteen months ago of seeing Sir David's beautiful new apparatus at work at the Cape. There was then no possibility of reversing the field of view during the transit; the wire went along with the star, and you tried to keep it bisected, and it seemed perfectly possible that with stars of different magnitudes you might have personality, though of smaller amount—the ordinary personality of bisection instead of that on a moving object. I understood then that there was going to be an interesting arrangement for turning the field of view round midway in transit, and have wondered whether this had been applied before the present results were obtained.

Dr. Halm. I think Mr. Hinks's suggestion that there is some trace of personality of bisection agrees with my own experience, but I must say that the amount of such personality must be very small. I had an opportunity of altering the micrometer of the transit-circle at Edinburgh in such a way as to convert it into a movable-wire micrometer; but the experiments have so far been limited to investigating the disappearance of relative personality between different observers. Two observers who had a personal equation of about 0·20 seconds by the old method of recording transits over fixed wires have now a personal equation of about 0·007 if I remember rightly, so that there is no doubt that the relative personal equation has been considerably reduced by the application of the method. But we have had no experience so far of investigating elimination of the personal equation with regard to magnitude. This is a piece of work which is now being undertaken and for which we have materials in the observations of the Zodiacal Star Catalogue, which have been partly taken by the old method and are now continued with the moving micrometer, and we hope to get a small contribution to the solution of that particular question by the comparison of the same stars that have been observed by the old method with the observations now taken with the revolving micrometer. I do not yet know how this investigation will compare with the beautiful heliometer method which has been brought before us to-night by Sir David Gill; but we are hopeful that we may find some results with regard to this important question.

Mr. H. P. Hollis. May I make a suggestion as to the actual reason of magnitude equation in transit observing? I have often thought that this is in a great measure due to the alteration of the lighting of the field for stars of different magnitude and a consequent re-focussing of the eyepiece. It is not an unusual thing for an observer to make an adjustment for focus in the case of bright stars different to that which he has for faint. It is not quite evident why this should alter the apparent position of the transit-wires with respect to the stars in the direction right or left, but some small experiments now being made at Greenwich on the collimation of a reversible transit—I am afraid I cannot here give details—go to show that even when the wires are placed in the focus of the object-glass as well as it is possible to put them, a small difference of position of the eyepiece—and there is always a small range of opinion in this respect—may make a sensible difference in the determined line of collimation.

Mr. W. W. Bryant. May I ask how the heliometer observations were made? Whether the image of the faint star was placed in the centre of that of the bright star, or whether it was made to touch it alternately to left and right: as in the first case the observation would seem liable to exactly the same sort of error, the error of bisection, as that found by Prof. Karl Pearson in his experiments. The greater part of what is called Personal Equation, the time element, depending on the quickness of perception of eye or ear, appears to be totally eliminated by the moving wire; what is left is a physiological error of bisection which differs for stars of different magnitudes. If a reversion-prism were used with the heliometer the objection disappears. [*Sir David Gill.* We always use the reversion-prism.] In that case there is no apparent personality left in the heliometer observations. The small error of bisection in the transits would alone remain. Even this is eliminated by the method used by the Potsdam observers, in which the direction of motion is reversed at the centre of the field, the second half of the transit being over the same wires as the first half in the reverse order. Without reversion there would always be slight differences due to bisection error.

Sir David Gill. I quite agree with what Prof. Turner says that these determinations of magnitude equation at the lower limit of magnitude are not deserving of much confidence, but I am inclined to believe that the last word has not been said on the subject, and I do say that the moving-wire method of transit-observing presents possibilities of arriving at elimination of the magnitude equation. In these observations which are before you, and which will be published in the next number of the 'Monthly Notices,' we had not the opportunity of using the most effective method of moving the wire automatically, and all the observations quoted in the paper were made by moving the R.A. motion by hand on the ordinary Repsold plan. We had a great deal of difficulty in getting the automatic apparatus to work. The rate of the electric motor that we had was variable, and it is necessary that the speed should be kept uniform within two or three per cent., because the acceleration or retardation given by pressing one or other of the regulating buttons only changes the velocity of the web's motion by four per cent. With a good clockwork driven by a weight which would give a nearly uniform velocity and with the use of a reversing-prism, I am sure that it would be possible to get rid entirely of personal equation. I quite agree with Prof. Turner that it is most desirable to determine the personal equation for all stars which are used as "standards" in photographic plates, and if anyone cares to present an 18-inch heliometer to the Cape Observatory I am sure they will be only too glad to observe magnitude equation by that method. I was glad to hear Dr. Halm, and I hope the Edinburgh Observatory will carry on the work of comparing observations by the old method with the moving-wire method. In that case I would suggest that Prof. Dyson observes the stars in the list printed in the paper; they are all stars equally observable from Edinburgh as from the Cape, so that one can compare them without trouble. With regard to how heliometer observations are made, you will remember probably that the bright star is reduced by the wire screen to an exact equality in appearance to the image of the fainter star, so that one cannot distinguish one from the other, then the distance-handle is moved till these two images seem to be immediately one above the other in the direction at right angles to the line of separation of the lenses; we then reverse the position of the images, in the sense top to bottom of the field and do the same thing in the opposite direction. The great point is that whether the faint star is preceding or following makes no difference at all, for the observer does not know which is which.

On the invitation of the *President* the Count de la Baume

Pluvinel addressed the Meeting. In a few graceful words he expressed the hope that he should meet some of the Fellows present at the forthcoming Meeting of the Solar Union in Paris.

The President then spoke of an observation which he made accidentally some years ago, which seemed to have a bearing on the possible cause of personal equation.

I was, he said, handling a direct-vision prism after looking at some faint star-spectra, and turned to look at a very small point of light on the tip of a reduced gas-flame in order to determine quickly which was the red end of the spectrum. The prism was of such construction as to give the green in the direct line of vision. As one does occasionally if one has been making tiring observations, I made a useless movement in an idle moment; under such circumstances often something declares itself of importance if one happens to have the luck to notice it. My idle movement was that I turned the direct prism round, with the result that the spectrum rotated in the field of view. Now if the impression were received equally quickly by all parts of the eye, one would expect that the linear spectrum would remain straight as it turns round the central point. As a matter of fact, I observed that it does not rotate as a straight line, but as a curved line, the violet and blue hanging back relatively to the green. The effect may be seen at whatever point of view one directs the eye. The complete interpretation of the phenomenon is a little difficult, and also, therefore, rather interesting. I enquired of Dr. Rivers, who is interested in the physiology and physics of the sense-organs, as to whether this observation had been made before, or anything like it, and whether he thought it was simply an effect due to the difference of colour falling upon the eye or to the difference of the distance from the *fovea centralis.* It is known, of course, that there is a difference in the rate of perception according as the light strikes the outer region of the retina or the inner. It is rather difficult to distinguish between colour-effect and the effect due to peripheral stimulation of the retina instead of the central stimulation. Dr. Rivers told me that Cornu, the distinguished French physicist, to whom we owe so many valuable observations, had observed the same thing in a simple form; he was working with a spectroscope with a vertical slit under circumstances in which there was an image of a narrow horizontal filament of a glow-lamp upon the slit. When the image was moved up and down with an oscillatory movement, he observed the same sort of results as I observed with the rotating prism, namely, the linear spectrum was seen to move not as a straight line, but as a curved line, the ends of the spectrum lagging behind *.

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* [*Note.*—I am not sure how far the facts have been completely worked out, but the bearing of the observation on the case of personal equation depending on magnitude is obvious, whether, as I attempted to indicate at the Meeting, we ascribe the retardation of the perception of faint stars to an excess of blue stars among faint stars or to a *difference* in the variation of the rate of perception with increasing distances from the *fovea centralis.*—H. F. N.]

The following papers were announced and partly read:—

W. F. Denning. "Early and Late Perseids."

Sir D. Gill and S. S. Hough. "Determinations of Personal Equation depending on Magnitude, made with the Transit-Circle and Heliometer at the Royal Observatory, Cape of Good Hope."

C. J. Merfield. "Determination of the Secular Perturbations of the Minor Planet Ceres, arising from the Actions of the Eight Major Planets of the Solar System."

W. E. Cooke. "The Electric Arrangements of an Observatory."

Royal Observatory, Greenwich. "On the Value of the Solar Parallax resulting from the Greenwich Photographs of Eros, 1900–1901." (Communicated by the *Astronomer Royal.*)

Radcliffe Observatory, Oxford. "Magnitudes of *o* Ceti, 1906 December 14–1907 February 16." (Communicated by the *Radcliffe Observer.*)

P. H. Cowell and A. C. D. Crommelin. "The Perturbations of Halley's Comet."

The following foreign astronomer was proposed by the Council as an Associate of the Society:—

Anders Donner, Director of the Observatory, Helsingfors, Russia.

The following gentlemen were elected Fellows of the Society:—

Arthur Neville Brown, M.A., Ludgrove, New Barnet, Herts.
Henry Cooper, 19 Cromer Road, Eastville, Bristol.
Phanindralal Gangooly, M.A., University, Calcutta, India.
William Newsam McClean, 42 Durdham Park, Bristol.

The following Candidates were proposed for election as Fellows of the Society:—

Major-General Henry Herbert Lee (late R.E.), The Mount, Dinas Powis, near Cardiff, South Wales (proposed by T. E. Heath).

Augustus Edward Hough Love, D.Sc., F.R.S., Sedleian Professor of Natural Philosophy, Oxford (proposed by H. H. Turner).

Rev. Reginald Wm. Bickerton Moore, M.A. Oxon., Vicar of St. James's, Bath, 11 Devonshire Buildings, Bath (proposed by Rev. D. Higham Sparling).

H. G. Tomkins, Examiner of Local Fund Accounts, N.W.P., and Oudh, Lahore, India (proposed by S. A. Saunder).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

SION COLLEGE, VICTORIA EMBANKMENT.

Wednesday, 1907 March 27.

F. W. LEVANDER, *President*, in the Chair.

Secretaries: J. G. PETRIE.
J. A. HARDCASTLE.

MR. PETRIE read the Minutes of the previous Meeting, which were confirmed.

The list of presents received was read by *Mr. Hardcastle*, and the thanks of the Association were accorded to the respective donors.

The name of one Candidate for Membership was read and passed for suspension, and the election by the Council of five new Members was unanimously confirmed.

Mr. E. A. Childe read a paper on "Glaciers and Solar Heat." He said that the possibility of our Sun being a variable star was of such interest and importance that any line of research which might possibly throw light upon it must be worth following up. His reason for bringing the subject up was that from the perusal of a large number of books of travel relating to polar regions and high altitudes an impression had forced itself upon his mind that, in the great majority of cases in which glaciers were mentioned, a recession within recent years was reported,.and that that was general in both hemispheres. The desiccation of great continental areas which had occurred during very recent geological periods and had continued during even historical times, would seem to show that they lived in an era of relatively small rainfall, and this would no doubt tend to diminish glacial action, apart from any possible falling-off in the Sun's heat. If, however, a record were kept for a long series of years, in which all available observations were recorded, it might in time prove of very great value. It would, of course, be the discovery of some variation of relatively short period, if such existed, for which one would hope, as the result of a systematic record such as he had suggested. Perhaps it might be thought that he had shown good reasons why that could not be of much value, but he believed that, notwithstanding all the difficulties of the subject, the record could not fail to be worth keeping, though to be of any great use it must, as he had said, rest on as broad a basis as possible, and it should therefore include the observations of travellers of all nationalities.

Mr. Maunder said it seemed to him that Mr. Childe had drawn attention to a very important subject, and that it would be well if something like definite statistics could be gathered as to whether glaciers on the whole were receding or not. There were one or two regions of the earth in which they seemed rather to be advancing. For instance, he believed that in Iceland there had been a very considerable increase in the extent of the glaciers within the last century, and the same had taken place in Greenland. But on the whole, whether glaciation was extending or diminishing, the evidence appeared to be very clear that there was a diminution of moisture of the planet. The evidence to which Mr. Childe had drawn attention seemed to be overwhelming that there had been within the last few thousand years a very marked diminution in the water expanse of our planet. Sir William Wharton, in his address to the British Association in 1905, incidentally referred to Patagonia, and suggested that an elevation of 2000 feet would cover the whole of that country with ice, and yet that the climate would not be very severe. The elevation of the land, therefore, by but a moderate amount would cause the entire country to be covered with glaciers. Thus they would have to look not merely to changes in the heat received by a given region and to the amount of moisture deposited over it, but also to its elevation, as to whether glaciers are increasing or diminishing. He feared, therefore, that from a strictly astronomical point of view such an inquiry as Mr. Childe had asked for would yield but little evidence as to a diminution or a change in the solar radiation even if they could ascertain definitely that the planet as a whole was emerging more and more from the glacial period; but nevertheless from the geological point of view it would be a question of the very highest interest to have fuller information on the subject.

The President said Mr. Childe had brought to their notice an inquiry of an original and very interesting character—one that would require very extended examination and observations to enable one to say anything definite on one side or the other. It was to be hoped that Mr. Childe, as well as others, would continue the investigation of the subject.

The President then announced that Father Cortie had kindly promised to give, at the meeting of the Association on April 24, a lecture on "Recent Spectroscopy at the Stonyhurst Observatory."

They were favoured that afternoon with the company of Mr. Eddington, one of the chief assistants at the Royal Observatory, Greenwich, who had kindly promised to tell the Association something about the researches he had made on the systematic motions of the stars.

Mr. A. S. Eddington then delivered his address.

Mr. Maunder said he was sure all the members of the Association would feel that they were very much indebted to Mr. Eddington for having come there to give an account of his most important investigations. The problem in which his paper marked a very distinct stage was the greatest of astronomical problems, that was to say, the question of the structure of the starry heavens. The problem went back as far as 150 years ago, when Thomas Wright, the son of the Durham carpenter, first brought out his suggestions as to the meaning of the Milky Way. Sir William Herschel, towards the end of the 18th century, took up the subject and worked at it from an observational point of view. It was not very long before he recognized a certain tendency in the proper motions of stars to show evidence that the solar system was moving through space; that is to say, the stars seemed, on the whole, to have a drift towards one part of the heavens and away from the opposite region. That idea Prof. Kapteyn and Mr. Eddington had shown must now be superseded by another and a more complex view. Two years ago Prof. Kapteyn brought out his evidence for a two-fold drift of the stars. Now Mr. Eddington had carried this considerably further, and had brought out a very remarkable and unexpected result—namely, that the stars of these two drifts appeared to be about the same average distance and about equally distributed in different parts of the sky.

Mr. Crommelin wished to re-echo entirely what Mr. Maunder had said as to the great interest and value of Mr. Eddington's paper. There was one point of detail on which he would like to ask a question. He gathered that in deducing the distance of the two drifts, the distance was got from the proper motion and that parallax researches had not been introduced. Mr. Eddington had told them that there was a tendency for drift II. to contain type II. stars. As our Sun was a type II. star, that would lead us to expect that our Sun itself was a member of drift II. That would tend to make the drift of stars of drift II. relative to the Sun smaller than that of drift I., because the Sun would share a part of the drift. In other words, if the drift were less, to make it appear equal it would mean that the stars of drift II. were relatively nearer than the stars of drift I. Might not that explain the apparent equality of drift which otherwise was so difficult to understand?

Mr. W. Heath said it seemed that the establishing of a double drift prevented one from continuing with the idea that the apparent motion was due to the Sun's actual motion. He wished to know whether the distribution of stars generally in regard to the direction of the drift was as if a whole ocean of stars was passing, or was it like a stream; and if the latter, was it towards a point of great density or from a point of great density? Had the greater part of the stream passed or was it still to come?

Mr. McCarthy asked what became of the old theory of the movement of the Sun in the direction of the constellation Hercules?

Mr. G. M. Seabroke said it occurred to him that the stars in the direction of the Sun's way would show, of course, less apparent motion due to widening out than stars further away at right angles to the Earth's way. Had Mr. Eddington noticed that the stars further away from the apex did show a distinctly steady increase of motion in the ratio named? He might put the question in this way: Were the motions on which the data were based as large in the Sun's way as they were at right angles to it?

Mr. Eddington said he was very glad the point brought forward by Mr. Seabroke had been mentioned, because it had reference to a rather strong point which he (the speaker) had omitted. Of course, in speaking of the value of the drift-motion, he was speaking of the component of the drift-motion *across* the line of sight. This vanished at the apex of the drift. Then as regarded the question as to whether the drift was denser in the direction from which it was coming or in the direction in which it was leaving us, so far as his own work went, he had not found any difference of density at all. Mr. Crommelin had mentioned the interesting point that our Sun was a type II. star, and this seemed rather to support the idea that it belonged to drift II., but the evidence in either case was very weak. As regarded the question, what became of the solar apex on this theory, the determinations of it are not really invalidated, but he thought it lost some of its interest through that cause.

The President wished on behalf of the Association to assure Mr. Eddington that they were all extremely grateful to him for his kindness in coming there to address the Meeting and lay before the Members the results of the very complicated subject he had been investigating.

Mr. Maunder exhibited and explained a number of photographs of recent sun-spots. A number of slides contributed by *Mr. A. M. Newbegin* were also shown and explained by *Mr. Thwaites.*

Mr. E. Holmes read a paper on "Jupiter and T.G."

Mr. Crommelin in reply gave on the blackboard the mathematical proof of the theorem of Lagrange.

The President said they were indebted to Mr. Holmes for bringing forward his paper, as it described the difficulties he had met with, and it had enabled them to have the benefit of Mr. Crommelin's remarks respecting it.

The Meeting stood adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE Monthly Meeting of this Society was held on Wednesday evening, April 17, at the Institution of Civil Engineers, Great George Street, Westminster, *Dr. H. R. Mill*, President, in the Chair.

A paper by *Mr. R. L. Holmes* on the "Phenomenal Rainfall in Suva, Fiji, August 8th, 1906," was read by the *Secretary* in the absence of the author. This is an account of a very remarkable fall of rain which occurred during a thunderstorm at Suva, the capital of Fiji, on the night of August 8th. Unfortunately the exact amount had to be, in part, estimated, owing to the observer failing to measure the fall at intervals during the night. Very little rain fell before sunset, but from 6 P.M. it continued a ceaseless downpour till sunrise the next day. At 10 P.M. the assistant found the gauge overflowing with 12·50 ins. of rain in it. Four hours later, at 2 AM., on the 9th, the gauge was again overflowing, and at 6 A.M. it was overflowing once more—that is, three times in 12 hours. Very little rain fell after 6 A.M. These measurements show over 37 inches, without taking into account the overflowings, which is an unknown quantity. As the gauge was 25 feet above the ground, Mr. Holmes is of opinion the rainfall should be increased by about 11 per cent., so that the total fall must have been fully 41 inches in about 13 hours, which he thinks surpasses anything that has been recorded in any other part of the world in so short a space of time.

Mr. R. Strachan read a paper on the "Temperature around the British Islands in Relation to the Gulf-stream." This was based on observations made in the year 1906, which have been published by the Meteorological Office. Around the British coasts the temperature of the air was lowest in February and highest in August; the temperature of the sea corresponded to these epochs with slight interruptions, having been lowest in January for the west and central, in March for the south, and highest in September for the north, and in July for the east positions. The water in the Strait of Florida was about 30° warmer than the sea at the north of Scotland.

Mr. L. C. W. Bonacina also read a paper on "Weather regarded as a Function of Climate."

The Planets and Planetary Observation.

[Continued from p. 134.]

MARS probably furnishes the most interesting planetary object of all for telescopic study. There may be differences of opinion on this point, for some observers are likely to prefer Jupiter on account of his large disc and changeable details, while others will select Saturn for his beautiful rings. The writer formerly considered Jupiter as the most attractive object, but an attentive study of Mars in the spring of 1903 caused him to regard the latter planet as decidedly entitled to preference. On Jupiter's expansive surface the markings are very distinct and may be watched through various transformations of aspect and alterations of speed—but they are merely atmospheric effects due to the great activity of, and frequent outbreaks from, the planet's hidden surface and to the currents of different velocity circulating around it. On Saturn there are similar phenomena, though they are comparatively seldom observed, owing to the much greater distance and smaller size of this planet. The noteworthy attraction is the ring presented at an ever-varying angle, but for general study there is not nearly the same abundance of detail as Jupiter displays. Both Jupiter and Saturn appear to be enveloped in dense, rapidly changeable atmospheres, affected in no small degree by the very disturbed and possibly heated condition of the surfaces below, which are always obscured by the vapours involving them.

But on Mars a very different state of things prevails. Here we discern stable markings, features of the actual globe. Our telescopes today show us precisely the same objects as were glimpsed by Fontana, Huyghens, Hooke, and Cassini about two and a half centuries ago. There is no dense atmosphere on Mars to curtain his ground formations or obscure his natural scenery, nor any very rapid rotation to drift such vapours as he may possess into bands parallel with the equator. What we see may be regarded as appertaining to the real surface, and as representing the outlines of continents, seas, snow areas, or, at least, regions of very different reflective power and very definitely bounded forms. And the repeated observation of these features gives us the exact period of his rotation, since they are constant in position and not disturbed by proper motions. Mars is indeed the only planet (apart from the Earth) whose rotation period is accurately known, and the best values place this at $24^{\mathrm{h}}\ 37^{\mathrm{m}}\ 22^{\mathrm{s}}{\cdot}65$, whereas Jupiter's spots yield periods from $9^{\mathrm{h}}\ 48^{\mathrm{m}}$ to $9^{\mathrm{h}}\ 56\frac{1}{2}^{\mathrm{m}}$, and Saturn's $10^{\mathrm{h}}\ 14^{\mathrm{m}}$ to $10^{\mathrm{h}}\ 38^{\mathrm{m}}$.

Mars not only displays permanent markings but a great abundance of them, and the configuration of his disc is as complicated as it is interesting. The features appear to be subject to certain temporary changes from time to time, but these are probably caused by Martian vapours, of somewhat limited extent and variable in position, overlapping them. At certain times well-known markings have become faint or exhibited deformation. These changes have, however, not been durable; the objects affected appear to have regained their previous outlines. And important changes occur in the apparent aspect of the planet due to the alteration in his inclination. Sometimes the N. pole is inclined to the Earth and the topography of that region favourably presented to observers, at other times we see the S. pole and the region surrounding it. At intervals of 15 years these changes are successively brought about; thus the planetocentric declination of the Earth and Sun with reference to the planet's equator were as under:—

1885 July	24°	North.
1892 December	25	South.
1899 July	26	North.
1907 December	26	South.

In 1914 the north surface will be again visible, while in 1922 the southern will be fully presented.

The apparent declination of Mars varies greatly at different oppositions, thus:—

Date of opposition.	Dec. °	Diameter. ″
1892 Aug. 3	23½ S.	29·4
1894 Oct. 20	8½ N.	25·6
1896 Dec. 10	25½ N.	16·6
1899 Jan. 18	24½ N.	14·4
1901 Feb. 21	14½ N.	13·8
1903 Mar. 28	0	14·6
1905 May 8	17 S.	17·4
1907 July 6	28 S.	22·8
1909 Sept. 23	4 S.	23·9

It happens that when the planet is nearest to the Earth his southern hemisphere is well presented, but at such periods it cannot be very effectively studied by European observers owing to his low altitude. Thus at the time of opposition in 1907 the planet will have an altitude of only 10° as viewed from Greenwich, and telescopic performance is not likely to be satisfactory under such conditions. In 1899, when the declination was 25° N., Mars was not far removed from aphelion at the time of his opposition and his apparent size was very small, though definition offered every possible advantage, the object being about 64° above the horizon when on the meridian.

The two hemispheres of Mars are notably dissimilar in their respective configurations. The southern presents large, irregular, dark patches with light areas and some brilliant spots. The northern exhibits a large number of dusky streaks and delicate lineaments extending over long ranges and apparently intersecting or interlacing in certain positions. These streaks are knotted in places and are by no means straight, very dark, and sharply outlined as they have been represented by some observers. They are veins of unevenly toned shading; some of them—as, for example, Protonilus and Cerberus—are quite conspicuous, while others are feeble narrow lines of so delicate an appearance that they can only be distinguished under a favouring combination of circumstances.

The streaks or veins, chiefly in N. hemisphere, of Mars are usually known as "canals," and the designation, though it may convey a wrong impression as to the real character of the appearances alluded to and may not properly describe their aspect, yet seems to be the best that can be applied. Of the objective existence of these canal-like markings there is no doubt whatever. Perhaps they do not cluster in anything like the abundance delineated by some observers, but the general system given in the charts of Schiaparelli is correct in the main. A few practical astronomers are unable to recognise the canals; hence there are still some sceptics who aver that these things are mere optical illusions, and, in fact, no more than products of the imagination. But the day is past for effective opposition of this kind; the actuality of the canals has been affirmed on evidence which it is impossible to controvert. It is perhaps strange that Dawes and other excellent planetary observers of the past did not detect these lineaments and include them in their drawings, but, except in regard to a few exceptionally conspicuous features of this kind, they appear to have seen nothing of them. There is no reason to conclude that the canals date their formation from the epoch when Schiaparelli discovered them thirty years ago.

One feature sceptics may very well take exception to, and their views are quite in accord with the best of recent observations. This is with regard to the alleged double canals. Schiaparelli appears to have duplicated nearly the whole canal-system in 1881–2 and to have portrayed the canals in conformity with his telescopic impressions: other observers, and some of them men of mark in the observational line, confirmed the gemination reported by Schiaparelli, but the peculiarity, though in favour for a time, has not survived the test of further experience. It may now be considered as one of the errors into which the critical practical studies of astronomers are apt to lead them. Fortunately such misconceptions are usually eliminated by the rigorous tests applied later, and though they cause much disputation and may temporarily divert the truth, they seldom exercise any serious check upon the progress of our knowledge.

[To be continued.]

The Sun-spots of 1907 *January.*

The New Year began with the great northern group, No. 6059—numbered by a typographical error in the *Observatory* for April, p. 176, line 26, as No. 6054—well advanced on the Sun's disc. This stream was, in its early days, as noteworthy for the beauty of its details as for its enormous size, but it simplified as it approached the west limb, the leader spot becoming more regular and the rest of the group fading out. As this group passed out of sight a yet greater disturbance, one of the most extensive ever photographed at Greenwich, came into view in the southern hemisphere. Its form was that of a gigantic irregular stream, but the spots composing it were not so closely compacted together as was the case with the chief groups of the last two years, and the total area covered did not equal that of the great group of February 1905. The component spots were therefore, without any great difficulty, arranged for measurement in four or five groups, viz., Nos. 6075, 6076, 6077, 6081, and 6085. Group 6076 seemed to be the chief nucleus of the disturbance and to represent the return of Group 6046. As this grand display passed off the disc, several large

Spot-Groups in 1907 January.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1907.	1907.	days.	1907. d	°	°	
6065	Jan. 1	Jan. 2	2	Jan. 6·6	71	+16	Small spot.
6	1	10	10	7·2	63	+ 9	**Return.** Regular spot.
7	2	6	5	1906. Dec. 31·1	156	+ 7	Pair of small spots.
8	2	6	5	1907. Jan. 1·3	141	−17	Small stream, *s.* Gr. 6062.
9	3	3	1	1906. Dec. 31·6	150	+12	Small faint spot, *nf.* Gr. 6067.
6070	3	8	6	1907. Jan. 6·1	77	−14	Irregular, unstable stream.
1	3	4	2	6·7	69	−12	Faint spot *f.* Gr. 6070.
2	4	7	4	1·6	136	+ 9	Small stream *f.* Gr. 6067.
3	8	8	1	4·4	99	+ 8	Few small spots *f.* Gr. 6059.
4	8	14	7	14·5	326	+13	**Return.** Regular spot.
5	8	20	13	14·3	329	−13	Regular spot.
6	9	21	13	14·8	322	−15	**Return.** Great irregular cluster.
7	9	21	13	15·0	320	−22	Large regular spot.
8	10	10	1	4·8	94	−10	Considerable stream.
9	10	13	4	9·5	33	+10	Straight sparse stream.
6080	11	12	2	8·2	49	+ 2	Small spot.
1	11	20	10	15·6	312	−18	Irregular stream *f.* Gr. 6076.
2	13	15	3	12·9	348	+15	Small faint spots.
3	13	17	5	15·7	311	+12	Small sparse stream.
4	15	15	1	17·9	282	+ 1	Small regular spot.
5	16	16	1	14·9	321	− 8	Small spot *n.* Gr. 6076.
6	16	20	5	21·5	235	+22	**Return.** Regular spot.
7	16	27	12	21·8	230	+19	**Return.** Regular spot.
8	16	27	12	21·8	230	−17	Fine straight stream.
9	17	19	3	22·6	220	−17	Small faint spot *f.* Gr. 6088.
6090	20	Feb. 1	12	26·1	174	+ 4	**Return.** Regular spot.
1	20	Jan. 21	2	15·3	316	− 4	Small spot. W. limb.
2	21	21	1	15·0	320	+ 2	Small spot. W. limb.
3	21	Feb. 1	12	27·4	157	+ 8	Large double spot.
4	22	Jan. 25	4	19·6	260	+19	Considerable stream.
5	23	23	1	22·0	228	+25	Small spots *n.* Gr. 6087.
6	23	24	2	26·7	166	+ 8	Small spots *f.* Gr. 6090.
7	24	25	2	28·8	138	+ 8	Pair of small spots.
8	25	Feb. 5	12	30·7	113	+ 8	**Return.** Regular spot.
9	28	7	11	Feb. 2·1	82	+22	Magnificent stream.
6100	29	8	11	2·7	74	+11	Fine stream.
1	29	7	10	4·3	53	+11	Large spot and train.
2	30	3	5	Jan. 28·1	147	+ 2	Small regular spot.

regular spots, mostly in the northern hemisphere, were seen, one of them being a return of Group 6059. But nothing at all comparable in interest to Group 6076 and its companions appeared until the last days of the month, when two magnificent streams, Groups 6099 and 6100, nearly on the same meridian in the northern hemisphere, came round the east limb, and January closed with the Sun's disc even more extensively spotted than when it began.

Rotation 713 began 1907 January 11·97.

Notes on the Principal Spot-Groups.

Group 6066.—Return of Group 6041. A small regular spot on January 1. It was crossed by a bright bridge on January 5, and a number of small unstable spots formed round it. Area on January 5 about 70 millionths of the Sun's visible hemisphere.

Group 6067.—A pair of small spots *f.* Group 6061. The group quickly developed into an irregular stream, much inclined to the equator. Area about 300 on January 4.

Group 6074.—Return of Group 6045. Third apparition. Final stage. A small regular spot, with occasionally a very small companion. Area about 15.

Group 6075.—A regular spot, with a few very small companions. Area about 70.

Group 6076.—Return of Group 6046. A great irregular cluster *f.* Group 6075. Area about 1000.

Group 6077.—A large regular spot *sf.* Group 6076. Area about 200.

Group 6078.—A considerable stream, appearing suddenly close to the West limb. Area about 250.

Group 6079.—A few small spots in a sparse but straight stream. Area on January 12 about 80.

Group 6086.—Return of Group 6051. Final stage. A small regular spot. Area about 20 on January 19.

Group 6087.—Return of Group 6053. A regular spot. Area about 70 on January 21.

Group 6088.—A few small spots developing into a fine straight stream, led by a large composite spot. Area about 400 on January 21.

Group 6090.—Return of Group 6061. A large regular spot. Area about 200 on January 26.

Group 6093.—A large double spot. The *n.* portion had broken up into a number of small spots by January 25. Area on January 23 about 300.

Group 6094.—A few small spots rapidly developing into a considerable stream. Area about 300 on January 24.

Group 6098.—Return of Group 6059. A large regular spot. Area on January 28 about 300.

Group 6099.—A magnificent group, composed of a very large spot of single nucleus, followed by a considerable train of spots, and with smaller attendants, principally to the south.

Group 6100.—A fine stream. The centre portion had disappeared by February 2, leaving two separate clusters. The leader is a large regular spot. Area of the group on February 2 about 800.

Group 6101.—A composite spot, followed by a small train of spots. The group had broken up into a cluster of small spots by February 4. Area about 180 on February 1.

E. Walter Maunder.

The Right Ascensions of the Eros Reference Stars.

With respect to the various sets of places that have been derived for the Eros stars, a few lines upon the magnitude equation of the Normal System that was computed here will serve to clear up the anomaly between the two lists, with the help of recent observations. For this system the observations available at the time were corrected for systematic errors depending upon the position of the star, and the results from thirteen observatories were weighted according to a simple scale, in which the general precision of each series (given by the average probable error) and the average number of observations were taken into account. Allowance for the existence of outstanding systematic errors, of unknown amount, prevented the use of weights, depending rigorously upon computed errors, derived from the precise number of observations for each star. The magnitude equation is an instance of such a systematic error.

A recent determination of the magnitude equation, at Lick, enables one to derive differentially the corrections to the Normal System, and also values for the magnitude equations of the separate sets of observations. In L. O. Bulletin No. 72, published two years ago, the differential comparisons are given in detail. The recent observations of the magnitude equation in both positions of the observer, facing north for stars north of the zenith, and facing south for stars in that quarter, indicate a decided difference in the amount of the correction, which had not been anticipated. The respective magnitude equations are found to be: North $-0^{s}\cdot025$, with some evidence of a rate at different parts of the scale; South $-0^{s}\cdot010$, with no sensible rate. This last result confirms the previous measures, which had all been made upon stars south of the zenith, and can be combined with the earlier results, for the corrections to the stars of List II., which were all observed here, facing south. The stars of List I. were all observed here facing north.

These corrections reconcile, very closely, the differences found in the comparison with the observations made with the clock-driven micrometer at Königsberg, and may be taken to confirm the relative freedom of the last from the effect of magnitude equation for at least a considerable part of the scale. While the material for absolute corrections is not nearly as extensive as would be desired, it appears to permit of a close approximation to differential values which represent actual observing conditions. The similarity of the separate magnitude equations in sign, and, for the most part, in size, is confirmed by general experience whenever the correction has been directly measured. The Lick correction has been independently confirmed by some late discussion of standard star-places. A recent comparison of the observations for the zodiacal catalogue with the Right Ascensions at the Cape Observatory, which had been corrected for magnitude equation, gives a slightly larger value for the Lick correction, but may be taken as an independent confirmation of the form of the equation. While a strictly linear form may not be the rule, and probably there is a variation at different parts of the scale for most observers, the use of a simple linear correction will undoubtedly reduce the effect very perceptibly.

The resulting magnitude equations for the Right Ascensions of Circular No. 11 of the International Astrographic Conference are:—

List I. North Stars.... Correction $-0^{\mathrm{s}}{\cdot}016$ per mag.
List II. South Stars.... „ $-0{\cdot}012$ „

Of the other systems of places for these stars, the one usually referred to as that of Lœwy appears to have been a provisional combination of the observations; no attempt was made to determine systematic or accidental errors, though separate results were weighted according to the number of observations. Its systematic differences from the Normal System are small, and the magnitude equations are practically the same as those derived above.

The system of Cohn, of Right Ascensions only, is virtually that of the series observed at Königsberg with the clock-driven micrometer. The other observations were reduced to this series, as to an absolute standard. Though one observation with this apparatus was only given double the weight of the best observations of the usual form, after these last had been reduced to the adopted standard; the weight of the one series at Königsberg has been computed to be practically equivalent to that of all the other thirteen combined (*A. N.* 4060, p. 50). According to the method of deriving the system of Right Ascensions, and with the assumed weight of Königsberg series (10 or 12), the comparison bears out the results of this computation; though the adoption of such a conclusion might be open to a question of judgment.

For the photographic system, the fact, to which attention has been called by others, that one series was rejected for its evident magnitude equation is an obstacle to the assumption that any or all other results of like character are entirely free from such effect. The comparison of several sets may show the absence of relative effect, while there may be magnitude equation in each. A similar conclusion might be reached by comparing some of the sets of visual observations. The magnitude equation appears to be of the same characteristic form for most observers by whom it has been determined.

With the use of the best corrections available, it will be found that the various systems will be reduced to fair accord. And it may happen that the discussion of this matter, in this connection, will lead to more general effort to measure this form of systematic error, so that the results of good observations may be utilized at their maximum efficiency. R. H. Tucker.

Lick Observatory, 1907, April 16.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Comet of 1006.

Gentlemen,—

Pingré gives a set of elements for a comet which he contends was seen in the year 1006, and these have a remarkable resemblance to those of Halley's comet. But there seems much doubt about the date at which it was supposed to have been seen. Riccioli, Lubienitzky, and others place it in A.D. 1200, but Pingré contends that it must have been 1006, because Haly-ben-Radouan gives the positions of the planets (calculated, not observed) at the time, which agree with the 30th of April in that year. It seems very difficult to determine the date of this writer (his name is more correctly spelt Ali-ibn-Rudhuán), who translated and commented on the 'Quadripartitum' of Ptolemy, whence a Latin translation was printed at Venice in 1484. Pingré says (vol. i. p. 224, under 'Titres des Citations') that he completed his translation in the year 530 of the Hegira, which corresponds to A.D. 1135. But how is this to be reconciled with what he says in p. 363, "Haly-ben-Rodoan étant jeune, on vit une Comète dans le quinzième degré du Scorpion: le tête étoit trois fois plus grosse que Vénus: elle rendoit autant de lumière que le quart de la Lune pourroit en donner." The date of this translation, if trustworthy, is of course also quite inconsistent with that of 1200 assigned for the comet by other cometographers. I hope that some Arabic scholar amongst your readers will be able throw some light on the matter, and determine his true epoch. But it seems to me that we must endorse Carl's remark about Pingré's orbit of the comet, viz., "Diesen Elementen ist jedoch gar kein Gewicht beizulegen." I cannot assent to Mr. Chambers's suggestion that it has been confused with the comet of A.D. 1066, which it seems we must now abandon as an appearance of Halley's.

Yours faithfully,

Blackheath, 1907, Apr. 5. W. T. LYNN.

P.S.—There is an amusing misprint in Pingré *in loco*. He tells us that at the time of the comet's appearance Mercury was "indivisible," a word from which the second syllable should evidently be deleted.

D_3 Absorption in the Solar Spectrum.

GENTLEMEN,—

Mr. Buss adverting again in your last January number to his observations of D_3 absorption happens to mention that he has never seen "what he could firmly believe to be D_3 in a spot umbra," and in support of it quotes Kodaikanal observations. I suppose he could have only meant that the *absorption-line* was absent, for we have had more than a dozen instances in which it was observed quite bright in the umbræ of spots. That the darkened line which is now of such common occurrence in the neighbourhood of certain class of spots rarely or never encroaches into the umbral regions, my own observations also go to confirm. For the last year and a half I have not come across any instance yet.

With regard to the absorption-line being found over facular areas, the fact was mentioned and instances were given in this Observatory Bulletin No. 3, which dealt with the observations made here up to the end of May 1905. And since the date of issue of that paper I have been paying particular attention to the regions this special absorption favoured. The first observations of dark D_3 were made in the neighbourhood of spots, and later observations go only to support that it has a decided partiality for these regions. To Mr. Buss's dozen occasions in which he has seen them over faculæ quite unconnected with spots, I could add many more, but as often have I been disappointed in looking for them over mere faculæ. I first thought it might depend on the brilliancy of facular veins, and not until after I failed to trace dark D_3 over some of the most brilliant of these did I give up the idea. From the first the behaviour of the hydrogen lines has been called attention to by all the observers in connection with this appearance of D_3, and here again the subsequent observations quite confirm the view that the state of C or F line was a good indication whether to expect D_3 absorption or not. But that there need be any very such great disturbances of the sort as I myself originally thought, or as Mr. Buss's letter now leads us to suppose, I am not prepared to say. Certain class of disturbances in the hydrogen lines which we should certainly call violent do not seem to go with the production of the dark line. I have watched some occasions in which the *dark* C line was violently agitated without, however, any reversal, and also two remarkable instances in which that line was displaced either way for several wave-lengths and yet dark D_3 did not appear at all. The particular sort of disturbance it would seem to favour I found to be *reversal* of the hydrogen lines, even the slightest of the reversals bringing out D_3 at once. I had not in the beginning considered the notched appearance in the C line as any disturbance at all, but now I think in overlooking them I missed a prolific source of the concurrent phenomena in this helium line. It is possible Mr. Buss, too, may have failed to note these very faint reversals, and hence may have arisen the many exceptions he has recorded as to the connection between disturbances in the hydrogen lines and D_3 absorption. Along with Mr. Buss I am also led to think that a slight provocation brings out the line, but I would add that the provocation appears to be the same as that which produces the reversals of the hydrogen lines. I have also found that the regions of small dots and penumbral patches which generally accompany the large spots are also favourite places of dark D_3, but in these the hydrogen lines are also often reversed, and even in cases where these reversals are not clearly present the disturbed condition of the region is well indicated by the way these dots show up suddenly and disappear as quickly.

Then the allied but certainly less frequent occurrence of bright D_3 in the umbra of spots connects the phenomena in the hydrogen lines and this helium line even closer. Now this bright D_3 on the disc of the Sun occurs, so far as my observations go, only in the umbra of spots; it abruptly stops short even at the penumbral spectrum. Again, I have seen it appear only when C or F were at the same time brilliantly reversed in the umbra of the spot. No matter however brilliantly reversed the hydrogen lines might be, if that were only outside the umbra, then the contrary phenomena of a dark D_3 at once appeared over the region. It is then very dark and quite sharp, while over the umbra as a bright line it is much broader. These are very suggestive phenomena and should be useful in elucidating some of the problems connected with the temperature of spots and even on the question of the regions concerned in spot-production. One supposition could possibly be that the hot erupted gases when drifting over the comparatively cooler umbra of a spot would produce the phenomena of the bright D_3, but that as against the still hoter photosphere it could only give rise to the absorption-line. But so many other phenomena connected with the solar envelopes will have to be taken into account.

With apologies for this rather long and rambling letter,

I remain,
Yours faithfully,
G. Nagaraja.

The Observatory, Kodaikanal,
1907, Feb. 25.

The Variations of η Argûs.

GENTLEMEN,—

When writing recently of this, probably the most remarkable of all the variable stars, it occurred to me that it was desirable to complete its history, if possible, to the present time. Perhaps many of your readers will remember that, in a letter to vol. xi. of *The Observatory*, Mr. Tebbutt stated that in April 1888 the star had increased in brightness by about half a magnitude from what it had been a year before, and also that its clear and sparkling light suggested the idea that it was preparing for another outburst. But as subsequent observations at the Cape showed no confirmation of this, but rather indicated that the star was below the seventh magnitude, I wrote to Mr. Tebbutt on the subject, and subjoin the paragraph relating to η Argûs, which shows that he has kindly again given special attention to it. The letter is dated Windsor, N.S.W., 1907, March 2, and the first part runs thus:—

"In accordance with the wish expressed in your letter of January 9th last, I have made comparisons of η Argûs with several stars in Sir John Herschel's Catalogue (Results, &c. p. 42). Careful comparisons on the evenings of February 16, 22, 23, and 24 last, by means of my 3½-inch refractor employed on former occasions, showed that the variable was very much less than Nos. 387, 522, 1183, and 1203, slightly less than Nos. 403, 404, 452, and 630, and about equal to Nos. 468 and 571. It certainly does not exceed the eighth magnitude. It is reddish, and its light very dull. On referring to my observatory Journal, I find that, in May 1888, the variable was very much superior to Nos. 403, 404, 452, 455, 458, 468, 575, and 630, considerably superior to Nos. 844 and 862, little inferior to No. 387, and considerably less than No. 522, and that its light was then remarkably clear and sparkling, very different from the present appearance. I think it has diminished by about a whole magnitude since 1888. There can be no doubt about the sudden but comparatively small outburst in that year."

Mr. Tebbutt adds that he has not for some years past attended systematically to alterations of this interesting variable, but he has often turned his eyes to the great nebula, so that if the star had reached naked-eye visibility it must have struck him. It would seem, then, now to be most probable that this star has exhausted its changes of brightness, and settled down into the status of an eighth-magnitude star, thus resembling to some extent 34 (P) Cygni, the Nova of 1600, which, after several remarkable changes, became apparently a permanent fifth-magnitude star.

Yours faithfully,

Blackheath, 1907, April 8. W. T. LYNN.

OBSERVATORIES.

Mount Wilson, California.—In his Report for the year ending 1906 September 30, Prof. G. E. Hale mentions two events which have bearing upon the future work of the Observatory—(1) the gift by Mr. J. D. Hooker of the means to provide a mirror of 100 inches aperture, and (2) the formulation of the hypothesis that the characteristic phenomena of the spectra of sun-spots may be accounted for on the assumption that the temperature of the metallic vapours within sun-spots is below that of the corresponding vapours in the Sun's reversing layer. The policy of the Observatory is to study the Sun as a star and to compare its phenomena with stellar phenomena. The first of the events above noted will give the means for studying faint stars; the second indicates the direction in which the Sun should be studied.

To come to the details of the work of the year. The Snow telescope, at first lent by, but which has now been bought from, the Yerkes Observatory, equipped with the large spectroheliograph and the Littrow spectroheliograph, has been used daily to take pictures of the solar surface in the light from calcium, and the heliographic portions of the flocculi are measured by means of the "globe-measuring" machine for determination of the solar rotation and other purposes. Besides this, daily photographs of the Sun are taken with the photoheliograph, and photographs of the spectra of sun-spots; in fact it is only necessary to say that the study of solar physics is carried on in every branch.

This Observatory is on the summit of a mountain accessible hitherto only by a trail. Arrangements have been made by the authorities for the construction of a road.

Harvard.—In his Report for 1906 Prof. E. C. Pickering again points out how great is the need of his Observatory for a considerable sum of money. It received a gift of $20,000 in 1902, which provided two large telescopes and a building for storing photographs, and since that date an attempt has been made to raise the sum of $50,000 for a library and to provide assistants for the study of photographs and publication of results. Though this attempt failed, Prof. Pickering continues to urge the necessity for these last items. The programme of work done in the past year was very similar to that of former years, photometry being the staple work. The work for which the 12-inch meridian photometer was constructed may be considered as substantially completed. With this and the 4-inch meridian photometer a standard scale of magnitudes down to the thirteenth has been made, and so many stars from the North Pole to declination −25° have been photometrically measured that a continuance of this work does not seem desirable. It is expected that the magnitudes of stars even fainter than this will be determined with the 60-inch telescope, but a method of determining magnitudes for faint stars photographically has been devised which may supersede this visual work.

The photometric observation of variable stars, the photography of Jupiter's satellite phenomena, and stellar spectroscopy have been continued as before. The reduction of the meridian observations of stars in the zone −9° 50′ to −14° 10′ is nearing completion.

Stonyhurst.—The contents of Father Sidgreaves's neat little publication for 1906 are mainly meteorological and magnetic. Astronomy is represented by the summary of the observations of sun-spots, which, compared with the magnetic records, go to show that the magnetic maximum lags behind the solar. The large Rowland grating spectrometer has been used on sun-spots on some days, but a new heliostat is being built for the observatory which will carry a 12-inch silver on glass reflector, and so make the grating more efficient.

NOTES.

Comet Notes.—A new comet (*b* 1907) was discovered by Mr. Mellish at Washburn Observatory, Wisconsin, U.S.A., the position being G.M.T. April 14^d 16^h 17^m, R.A. 6^h 40^m, N. Dec. 8° 0′, daily motion $+12^m$, N. 7°. It is clear from the very rapid motion that the comet was near the Earth, and as its magnitude was only 11·0, it will not develop into a conspicuous object. The following orbit is by Messrs. Lamson and Frederick from observations on April 15, 16, 17:—

T	1907 Mar. 27·56 G.M.T.	
ω	328° 47′	1907·0.
☊	189 7	
i	110 12	
log q	9·9657	

	R.A. h m	N. Dec. ° ′
April 30	8 17	44 17
May 4	8 28	46 30
8	8 37	48 5
12	8 45	49 14

The brightness is rapidly diminishing, and at the beginning of May is only $\frac{1}{10}$ of that at discovery.

Ast. Nach. 4157 contains the following elements of Comet 1906*g* by George Dybeck, from observations on Nov. 19, Dec. 17, Jan. 14. The middle place gives a residual of $-25''$, so that possibly there is sensible deviation from a parabola:—

T	1906 Nov. 21·3371 Berlin M.T.	
ω	$8^\circ\ 42'\ 44''$	1907·0.
☊	84 56 21	
i	56 33 39	
$\log q$	0·084409	

A. C. D. C.

MINOR PLANET NOTES.—There has been a great haul of planets during the last month or two. Dr. Metcalf has discovered the following at Taunton, Mass., U.S.A.:—XX, March 6; XY, March 7; YA, Feb. 15; YB, March 7; YC, March 7; YD, March 9; YE, March 11; YG, March 11.

M. Liapin has found at Pulkova XZ on March 18.

Prof. Lowell has found at Flagstaff, Ariz., U.S.A., YF on Feb. 12.

The remainder were found at Heidelberg:—YH, Kopff, April 2; YJ, Kopff, March 21; YK, Lohnert, April 3; YL, Kopff, April 3; YM, YN, YO, YP, YQ, YR, YS, YT, YU, YV, Kopff, April 4; YW, YX, Kopff, April 5.

XL is identical with 84 Clio; 462 Eriphyla was close to this, and some of the images reported as XL belong to Eriphyla.

YW, YJ may be a single planet, but this is not likely.

YW was very near Comet 1905 IV., the magnitude of the latter being 13·6. XZ may be identical with 469 Argentina, and YL with XU.

A. C. D. C.

THE LIMIT OF VISION WITH DIFFERENT APERTURES.—Some facts on this subject are incidentally given at the end of the volume of observations of variable stars made by Mr. J. A. Parkhurst during the years 1894 to 1906, referred to by Prof. Turner at the R. A. S. Meeting in March. Mr. Parkhurst's object was to make some researches in stellar photometry, among them being the accurate determination of complete light-curves of twelve variable stars of long period having faint minima, and especially the behaviour of the stars during their faint stages, when they can be seen only with the largest apertures. The observations were made with an equalizing wedge-photometer on three different telescopes, a 6-inch reflecting Newtonian telescope, and the 12-inch and 40-inch refractors of the Yerkes Observatory, which Mr. Parkhurst had the privilege of using. The results of the observations of these stars which are given in this volume* can scarcely be abstracted here, but the *résumé* at the end gives some information about the precision of variable-star observations. Mr. Parkhurst compared the star under observation with several stars in the field whose magnitudes had been determined either at Harvard or at Potsdam. When comparing with Potsdam stars the mean residual for different fields ranged from 0·03 to 0·11 of a magnitude, whilst comparing with the Harvard measures the mean residual ranged from 0·05 to 0·28. From this the author argues that the Potsdam photometry is more accurate than the Harvard, and that his own measures approach those made at Potsdam in precision. As to the faintest star that can be seen with each telescope, according to Pogson's formula *, the limiting magnitudes visible with a 6-inch, a 12-inch, and a 40-inch should be respectively 12·89, 14·40, and 17·01. The actual observed magnitudes were:—

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* 'Researches in Stellar Photometry,' by J. A. Parkhurst, S.M., Instructor in Practical Astronomy in the University of Chicago. Published by the Carnegie Institution of Washington.

	Harvard scale.	Potsdam scale.
6-inch, power 150	12·90	13·02
12-inch, power 275	14·27	14·57
40-inch, power 750	16·8	17·1

THE SOLAR MOTION RELATIVE TO THE FAINTER STARS.—In No. 591 of the *Astronomical Journal*, Prof. Comstock makes an investigation with the proper motions of 216 stars between the eighth and twelfth or even thirteenth magnitude as data. Results that he arrives at, among others, are that:—

(1) The mean distance, and therefore the mean luminosity, of these stars is considerably less than has been commonly assumed.

(2) The proper motions, and presumably the parallaxes, of stars fainter than the ninth magnitude show a progressive increase with increasing distance from the galaxy.

(3) The motion of the Sun relative to the more distant stars (10th and 11th magnitude) is not widely different from its motion relative to the brighter lucid stars, and the flux of the nearer parts of the stellar system with respect to those more remote is therefore small.

ASTRONOMICAL REFRACTION.—A paper by Prof. H. G. & de S. Bakhuyzen in the 'Proceedings of the Amsterdam Academy of Sciences' (January 26) suggests that it may soon be thought necessary to revise the theory on which the correction for refraction is computed, in view of new knowledge as to the temperature of the upper air derived from observations made by means of balloons. The assumption underlying the accepted laws of refraction is that the atmosphere consists of concentric spherical layers, whose common centre is the centre of the Earth, and whose temperature decreases as the radius increases. Balloon observations show that the temperature does not always vary in this way, but that at some heights the temperature rises as the distance from the Earth increases, and for this reason it is thought that the law of refraction may require some alteration.

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* Limit of vision = 9·0 + 5 log (aperture in inches).

The Observatory at Nice, which was founded by the late M. Raphael Bischoffsheim in the year 1881, and has since been supported entirely and liberally by him, has now passed, according to bequest, into the proprietorship of the University of Paris, and will be carried on as an establishment of the State. The first Director of this Observatory was M. Perrotin, the second and present one is General Bassot, a distinguished geodesist, formerly Chief of the Service Géographique de l'Armée, and a Member of the Bureau des Longitudes.

Dr. Doberck informs us that he is retiring from the Directorship of the Hong Kong Observatory, having occupied that position since 1883. His successor will probably be Mr. Figg, who has been Dr. Doberck's First Assistant. As the Chief Assistant, Mr. J. I. Plummer, was elected to Fellowship of the R. A. S. in 1876, he is no doubt nearly of an age when retirement is necessary.

The rainfall at Greenwich during the past month (April) amounted to 3·14 inches, which is exactly double the average (1·57), and has only been exceeded four times in the last sixty-seven Aprils, and not at all in the fourth month of the year since 1878. The four years in which the total was greater were 1848 (3·44 in.), 1853 (3·21 in.), 1877 (3·35 in.), and 1878 (4·31 in.). A notable meteorological feature of the month was the contrast between April 24 and 26. On the first of these days the maximum temperature was 53°, which is the highest for that date in the Greenwich records. On April 26 the maximum only reached 45°·5, which is the lowest for the day since 1857.

The third Meeting of the International Solar Union will be held at Meudon, France, in the week beginning Monday, May 20.

It has been found necessary to postpone the second part of Prof. Herschel's article on "The Great Fireball of October 7, 1868," owing to a rather serious illness of the author, from which, we are pleased to say, he is satisfactorily recovering.

The death is announced of Prof. J. K. Rees, late Director of the Columbia College Observatory, New York.

THE next Meeting of the Royal Astronomical Society will be on Friday, May 10; of the British Astronomical Association on Wednesday, May 29.

FROM AN OXFORD NOTE-BOOK.

THE value of Chandler's Catalogues of Variable Stars is so great that no apology is necessary for recording any printers' errors in them. The following notes of cases of inconsistency between the Calendar and Julian Date in the Third Catalogue (*Ast. J.* No. 379) are due to the industry of Miss M. A. Blagg:—

Star's No.	Calendar.	Julian.
22	1890 Sept. 30	= 2411641 *not* 640
107	1871 Mar. 31	= 2404518 *not* 515
715	1872 Mar. 22	= 2404875 *not* 867
4377	1861 Apr. 26	= 2400892 *not* 891
6170	1890 Feb. 1	= 2411400 *not* 412
7444	1864 Sept. 16	= 2402131 *not* 133
7783	1890 May 6	= 2411494 *not* 484

Of these, Nos. 107, 715, 4377, and 7444 are repeated, either directly or indirectly, from the Second Catalogue (*A. J.* 300). The First Catalogue contains no Julian dates; and it thus seems possible that the Calendar dates, and not the Julian, are to be taken as correct in cases where there is a discrepancy; but Mr. Chandler's ruling on this point would be welcome.

◆

MAY I ask whether any variable star observer has any records tending to throw light on the possibility of a slow variability in BD $+16^{\circ}\cdot4208$, 2^{m} 52^{s} following and $1'$ $56''$ N. of R Sagittæ? The BD magnitude is 8·0: in Berlin A (No. 8073) the magnitude is given as 7·2 with a special note of the discrepancy from BD; and Baxendell's comparisons with R Sagittæ and other stars in the years 1878–1881 assign a magnitude of 8·9, though there is just a chance that the identification may be incorrect. The "Harvard sky" also makes B.D. $+16^{\circ}$ 4208 distinctly fainter than either $+16^{\circ}$ 4196 or $+16^{\circ}$ 4177, while the Berlin magnitudes are 7·2, 8·0, and 7·5 respectively, and the B.D. magnitudes 8·0, 8·2, and 8·2.

IT is most gratifying but rather startling to receive a letter from "an *old* and ardent reader of the Oxford Note-Book"; and yet on looking up the records one finds that the accusation of age has a basis of fact: for this heading first appeared in November 1894, nearly thirteen years ago. One advantage of looking up such references is that other things catch the eye incidentally. In this particular case the "other thing" was the graphical method of solving a spherical triangle, suggested by M. Radau and sketched on p. 188 of this Magazine for 1894. It is pertinently pointed out, at the bottom of the page, that the method is hampered in practice by the great size of the requisite diagram; but it seems possible that this disadvantage may be got over, at any rate for a certain class of cases. Let A be the pole of the heavens, C the centre of a photographic plate, B any star on the plate. The distance CB will be small—say less than 10°. Given the angle C and the sides a, b, of which a is small, we can find the side c graphically by the following method, which will be readily recognized as an adaptation of Radau's:—

Suppose a semicircle divided into degrees, and from each division a line drawn perpendicular to the base; the base may be said to be graduated by the law of cosines. Let the base be a long tape: or better still, perhaps, cut it up into sections, each of which may be on its own arbitrary scale, but is divided throughout its own length on the cosine law.

If a is small, the angles $(b-a)$ and $(b+a)$ are not very different, and the points D and E on the scale corresponding to them will fall near together, even though the whole scale is very long. Describe a semicircle on DE, centre F; and taking the point G so that the angle GFD = C draw GH perpendicular to DE. Then the reading of the scale at H is the value of c in the formula

$$2 \cos c = \{\cos (b-a) + \cos (b+a)\} + \{\cos (b-a) - \cos (b+a)\} \cos C.$$

Of course we need not actually describe the semicircle on ED; all we need is a short converging scale divided according to the cosine law, the ends of which can be adjusted to the points D and E.

This digression has, however, taken us away from the "old and ardent reader," whose object was to draw attention to a thrilling tale in *Pearson's Magazine* for April entitled "A Message from the Moon." Some clever inventors find a way of throwing a powerful search-light on to the Earth-lit portion of the crescent Moon, and use it for purposes of advertising. A startled world reads the legend "Use Moon Soap" on the usually dim face of our satellite; for about 3½ hours this bewildering apparition remains and then the letters seem to run together and disappear, the fact being that the search-light apparatus has fused and caught fire. The story is well told and worth reading. What caught the attention of my correspondent was the impossible picture of the Moon, which has the illuminated crescent on the upper left side: so that the illuminating Sun would be about on the meridian and high in the heavens. It is curious how prone the artist is to err in such cases.

The story suggests a good examination-question for those who are in need of such things, viz.:—

1. "Explain why it is difficult to throw an illuminated advertisement on to the face of the New Moon."

Or perhaps:

2. "What should be the approximate brightness of an earthly search-light of given dimensions in order to illuminate the Moon as brightly as the Sun illuminates it?"

But these are questions suitable for "honours" candidates rather than for "pass." Here are some easier ones:—

3. A whale over seventy feet long was found by the steam-trawler 'Cetus' in a dying condition in the North Sea, and was towed into port at Grimsby, a distance of 100 miles. (*Daily Mirror*, 1907, March 11.)

Can you trace any connection between the death of the whale and the heliacal setting of the constellation Cetus?

4. Comment on the following passages:—

(*a*) From 'Lavengro,' by George Borrow (chap. lxx.):

> But how different must life be in Jupiter from what it is in our little star! Life here is life beneath the dear Sun—life in Jupiter is life beneath the moons—four moons—no single moon is able to illumine that vast bulk. All know what life is in our own little star; it is anything but a routine of happiness here where the dear Sun rises to us every day: then how sad and moping must life be in mighty Jupiter, on which no Sun ever shines, and which is never lighted save by pale moonbeams!

(*b*) From 'Jane Eyre,' by Charlotte Brontë (chap. i.):

> [The Moon's] newly risen crescent, attesting the hour of eventide.

5. Compare the ideas of "personal equation" indicated in the following utterances:—

(*a*) Of a chairman introducing an astronomical lecturer in the words: "I have never heard of the personal equation in astronomy, but we are here to have the opportunity of enjoying it. Your lecturer at any rate does not intend to be the unknown quantity in that equation."

(*b*) Of a lady meditating (Lady Calmady in Lucas Malet's 'Sir Richard Calmady') as follows: "She felt that this was an essential part of her being—the unit of her personal equation."

It is not intended to invite answers to the above questions; but I should be really interested to get solutions to the following problem. It is taken from a most interesting paper by Prof. J. McKeen Cattell, of Columbia University, entitled "A Statistical

Study of American Men of Science" (see *Science*, n. s. xxiv.). The idea of getting scientific men to arrange one another in order of merit and comparing the results so as to get the probable error of an estimation is so novel that there is a human tendency to laugh at it in the first instance. But everything depends on the way in which the project is carried out, and after seeing Prof. Cattell's work one realizes that such an investigation may be of real value. From much that is interesting in his paper I extract a table of particular concern to astronomers, representing

The order assigned to ten [U.S.] astronomers by ten [U.S.] observers.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
A	1	2	4	3	10	6	9	5	11	8
B	1	4	2	5	6	?	9	3	8	7
C	1	4	?	5	2	16	6	17	7	21
D	?	2	4	3	1	5	7	13	8	6
E	1	9	2	5	6	3	8	4	7	11
F	1	4	10	2	5	6	3	7	8	11
G	1	3	5	16	2	6	7	13	4	8
H	1	3	5	7	6	4	9	?	8	2
I	1	2	8	4	10	6	7	3	11	5
J	1	2	4	5	12	8	3	6	13	7
Mean...	1·0	3·5	4·8	5·5	6·0	6·6	6·8	7·8	8·5	8·6

The table is not difficult to understand. The ten observers are A, B, C . . . J; and from their lists of *all* U.S. leading astronomers the ten [U.S.] leading astronomers I., II., III., &c. were determined, on a tolerably obvious principle indicated by the row of means. As regards the query-marks, the note is that the observing astronomer did not assign a position to himself.

Now my problem is, *with this information before us*, to assign the identities of the astronomers I., II., III., ... X. This is in two essential respects a different problem from that set by Prof. Cattell: firstly, we have the information before us, and, secondly, the European reputation of these astronomers (who, it may be repeated, are all American) may differ sensibly from their "honour in their own country." If a sufficient number of my readers will kindly take the trouble to make out their lists, I venture to think the results will be of interest, if not of value; and I should propose to send them to Prof. Cattell for his comments, after a preliminary and unskilled attempt at reduction. Is the request too alarming?

FROM *Punch* for April 3, 1907 :—

GARDENER. Good morning, Master George. I hear you won the 100 yards at the Sports.

GEORGE. Yes, Carver, but in very poor time—only 12⅖.

GARDENER. Ah, the best time I ever did the hundred yards in when I was a boy was ten seconds.

GEORGE. I suppose they timed you with sun-dials in those days?

FROM an article on "The Greek Anthology" in the *Nineteenth Century* for April (p. 633):—

The lines of Ptolemy the astronomer on himself, though not a sepulchral inscription, are full of immortality, and strike perhaps the sublimest note in the Hellenic gamut. "I know," says Ptolemy, "that I am mortal and the creature of a day; but when I search out the many rolling circles of the stars, my feet touch the Earth no longer, but with Zeus himself I take my fill of ambrosia, the food of the gods."

* * * * *

Of all the epigrams attributed, rightly or wrongly, to Plato, the most famous has been expanded by Shelley into the four glorious lines:

Thou wert the morning star among the living
  Ere thy pure light had fled:
Now, having died, thou art as Hesperus, giving
  New splendour to the dead.

ONE of the great observatories in the United States received recently the following protest against our astronomical symbols. There are indications (*e. g.* in the adopted pseudonym) that the writer is "assuming a" virtuous indignation and "has it not"; but it seems desirable to consider his views on the offchance of a true word being spoken in jest:—

DEAR SIR,—

The enclosed clipping is from the New York Press of this date. I want to add my protest to the assinine, selfish way you so-called astronomers publish the location of so interesting an object as a comet. If you had any decency or interest in elevating your fellow beings with intelligence of such interesting objects, you would state their location so that those who are not blessed with telescopes could at least seek and perhaps, some might, find them with the aid of glasses not so strong as are yours.

Until all the world becomes as wonderfully intelligent (?) as your ilk, it is to be hoped that either they or you will soon die, and that good instruments will fall into the hands of those who will give *intelligently* the location of newly found comets.

There are hosts of people not nearly so INTELLIGENT as you are who would, *without an effort*, be kind enough to let THE PEOPLE know where to look for a new comet, by locating it by familiar stars and constellations, and not by the unknowable "right ascension 6 hours 58 minutes," etc.

Why don't you die or resign and give your job to somebody with a little common decency and a great deal of common intelligence.

Yours,
T. I. RADE.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. JUNE, 1907. No. 384.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1907 May 10.

Mr. H. F. NEWALL, M.A., F.R.S., *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder. Sixty-nine presents have been received since the date of the last Meeting. Attention may be called to two volumes of the Harvard Annals, presented by the Observatory, one on Eclipses of Jupiter's Satellites, the other a Catalogue of Variable Stars; also to a book, 'Mesures d'Etoiles doubles,' presented by the author, M. Farman, and 20 enlarged reproductions of Astrographic Chart prints, presented by the Royal Observatory, Greenwich.

The President asked Mr. Maunder if he could give an account of the paper on sun-spots presented by Mrs. Maunder.

Mr. Walter Maunder. The present paper is by Mrs. Maunder, and was begun by her nearly eighteen months ago, but pressure of other duties greatly hindered the prosecution of it. It is founded almost exclusively upon the Photoheliographic Results published in the annual volumes of Greenwich Observations. These results are given in two forms, "Daily Results" and "Spot Ledgers." The Spot Ledgers alone are used in this paper, and only the numbers in the fifth and eighth columns of the ledgers. The fifth column gives day by day the total area of each spot-group, corrected for foreshortening and expressed in millionths of the Sun's visible hemisphere. The eighth column gives, for the centre of gravity of the group, its longitude from the central meridian, day by day. As the mean synodic rotation period of the Sun is nearly 28 days, a given spot-group may be observed on 14 consecutive days, and the apparent daily motion of a group is in the mean 13°·2 in solar longitude. If the visible hemisphere of the Sun be therefore divided into lunes, 13°·2 in breadth, starting from the central meridian towards the east and towards the west, we shall have the hemisphere divided into 14 divisions, the first and last of which are a little narrower than the other twelve. Indeed, as a spot is practically invisible at 86° from the central meridian, the first and last lunes will only be half the breadth of the others. A spot-group remaining under observation for 14 days would therefore appear in a different lune each day. The preliminary work for the paper was to write down the area of every spot-group on each day, in the column set apart for the lune in which it was seen upon that day. As sun-spots may be seen on only a single day, or may be observed for 14 consecutive days, or for any interval between the two, they naturally divide themselves into 14 different classes, as groups of 1-day duration, 2-day, 3-day, and so on.

Mrs. Maunder, in the present paper, has limited her enquiry to the sun-spot cycle 1889–1901, Greenwich ledgers not being yet available earlier than 1886. When the total areas for these 13 years were summed up, lune by lune, it was found that there was a steady rise observable from the first lune to the seventh, the eighth lune gave nearly as great an area as the seventh, and the succeeding lunes steadily diminished to the fourteenth. Several factors tend obviously to somewhat increase the areas of the lunes close to the central meridian over those more distant from it: irradiation giving an exaggerated value for the solar diameter; refraction in the solar atmosphere; the fact that in the process of measurement the spot appears as a marking on a plane surface, not as on a sphere; all tend in the same direction, namely, to make the correction applied for foreshortening a little smaller than it should be. But all such sources of error are perfectly symmetrical about the central meridian, as are also sources of error in the actual measurement; they do not explain, therefore, a remarkable circumstance which these total areas display, namely, that the total area for any eastern lune is greater than for the corresponding western lune.

The next step in the paper was to discard the areas of the groups and to consider only the numbers of groups observed, large and small groups being thus considered of equal value. Here an exactly analogous phenomenon was observed; the number of groups in each lune rose steadily from the first lune to the seventh and diminished from the seventh to the fourteenth, the total for any eastern lune being greater than for the corresponding western lune. Indeed the difference was proportionately on a larger scale than in the case of the areas.

The question now suggested itself as to whether most spots form in the visible hemisphere or in the invisible. Mrs. Maunder shows in the paper that, whereas 947 groups were first seen close to the eastern limb, only 777 were last seen close to the western limb, so that 170 groups appear to have been eliminated in the visible hemisphere. The difference was most marked in spots of considerable duration; groups lasting from 8 to 11 days, and first seen close to the eastern limb, were nearly twice as numerous as those of similar duration last seen close to the western limb. Dealing with the question of groups that obviously formed or dissolved on the visible hemisphere, a yet more remarkable condition was brought out, namely, that the number of formations decreased lune by lune almost steadily from the third lune to the twelfth; the number of dissolutions increased almost steadily from the third lune to the twelfth. But the number of dissolutions was considerably greater than the number of formations—the death-rate was higher than the birth-rate.

Returning to the question of areas, and dividing the groups into classes according to the number of days that they were under observation, the curves for the total areas for each lune, for each class of groups taken separately, were different for each class. Spots of 14-day observation gave a nearly regular curve, with a slight maximum to the east of the central meridian. The shorter duration spots showed a steady tendency to move the maximum towards the west and to make it more accentuated. Mrs. Maunder explained the cause of this gradual alteration of the form of the curve by the action of what she has termed "spot phase." Thus, for example, if we consider the case of groups observed for 8 days, we see that some of these groups are true 8-day groups, born in the visible hemisphere and dying there. We know that this is the case where a group is first seen not earlier than the third lune, and last seen not later than the twelfth. These we may term "Central" groups. But groups first seen close to the east limb may have formed in the unseen hemisphere, and may have had a long existence before they came into view; they therefore may not be true 8-day groups at all. Similarly, for groups last seen close to the west limb: if we separate the 8-day groups into these three categories, we find at once that a great number of Eastern groups were born in the unseen hemisphere, and we only saw their last declining days; probably quite two-thirds of them were of this description. The Western groups were much less numerous, only about one-third as many of these probably passed beyond the west limb as had come into view at the east limb. But these groups, few comparatively as they were, were observed during their days of most rapid growth, and consequently their mean area per group came out as about four times as great as for the Eastern groups.

But all effect of "spot phase" must go out when we are considering the totality of all spots, unless we assume that there is some connection between "spot phase" and the position of a spot in its presentation to the Earth; in other words, that the life-history of the spot is affected by such presentation. It is possible also to select groups in which the effect of "spot phase" should be practically negligible. Not a few groups are observed in more than one rotation. If we take the cases in which a group was observed in at least three rotations, and discard the first and last apparitions of the group, then we are sure that we are dealing neither with the period of rapid growth nor with that of final decay. But here again the areas give us in the totality of all spots available a steady excess, lune by lune, of the eastern hemisphere over the western, an excess which is seen to be greatest halfway from the limb to the centre.

Both in numbers and in areas, therefore, the spot-groups of the eastern hemisphere show a marked preponderance over those of the western. When the paper was so far completed, it occurred to Mrs. Maunder to enquire whether any similar preponderance had been observed in other directions, and she came across a short paper written in 1897 by Prof. Sykora, who noted that prominences were somewhat more numerous on the eastern limb of the Sun than on the western in the years 1880–1896. She took out the numbers of prominences observed at the Catania Observatory up to and including the year 1905, and found that the eastern hemisphere had the advantage until the year of minimum 1901, then the western, until 1905, when the eastern was again the more prolific, but during the years which her paper includes there was a steady superiority of the eastern prominences.

The title Mrs. Maunder selected for the paper is that of "An Apparent Influence of the Earth upon the Numbers and Areas of Sun-spots." The statistical facts brought out show a distinct influence of the Earth upon those numbers and areas as counted and measured, because the lunes and quarter-spheres into which the Sun's visible surface has been divided for the purpose of these statistics have no existence apart from the Earth; they are defined simply by the Earth's position relative to the Sun. But this influence may possibly be observational only, *i. e.* one of standpoint. Therefore she has added the word *apparent*, for it is conceivable that there may be something in the physical structure of a sun-spot or of its surroundings which renders it better seen when coming towards the Earth as brought by the solar rotation from the east limb to the central meridian than when receding from the Earth, from the central meridian towards the west limb. A heaping-up of the photosphere in the rear of sun-spots might tend to produce such an effect, or a canting backwards of the vertical axis of the spots themselves. But from either point of view these statistics bring out a new and very important fact; they indicate that either sun-spots in general have this dissymmetry in their construction, or else the Earth has some real influence upon the Sun's surface, tending to veil and extinguish spots and to check their production.

Dr. Lockyer. I should like to congratulate Mrs. Maunder on the interesting statistics she has brought before us. I think I pointed out two or three years ago that spots always preceded, or at any rate formed the head of a mass of flocculi, and in that way one side of a spot may be humped up. That, I think, is one possible explanation of the observed fact.

The Astronomer Royal. I also would offer my congratulations on the interesting paper by Mrs. Maunder. Whatever may be the explanation of it, it seems to me she has established a fact which is of great importance as to the behaviour of spots in crossing the disk. I had the advantage of having her own lucid explanation of the paper the other day, and I was much struck with it. I must express my gratification that she has made such good use of the Sun-spot Ledgers which have been accumulating at Greenwich for a number of years.

Prof. H. H. Turner. We have another interesting paper from Mr. and Mrs. Maunder which seems to follow very quickly after the other surprise of a very interesting kind that Mr. Maunder put before us about magnetism and solar rotation. I join heartily in the applause and congratulations on this paper, and if I say anything in the nature of an objection it is in the full expectation that it will draw from Mrs. Maunder some explanation. It arose from the train of thought suggested by the simile of birth-rate and death-rate. Now in one sense we may say the death-rate does not equal the birth-rate, because a very large man may die, but it was a little baby that was originally born. If it went by areas, the two rates would not be equal, so that in contemplating the human race one would see a continual increase in size represented by the growth of each man. There is no corresponding gradual decrescence, because the big man goes out suddenly. A sun-spot history is the reverse. There is a sudden rise, not so sudden as the decline in the case of the man; it is gradual compared with sudden death, but still it is not so gradual as the decrescence. Rise is quick and the fall is slow. Can that in any way be an explanation of the phenomenon? It is obvious that we are concerned simply with the average rate of growth in the case of men and women, but with decrease and increase of varying rate in the case of sun-spots; it is a life-phenomenon and not the phenomenon of a particular moment that we should observe. I do not know whether I have made this objection, or rather question, clear. It is possible Mr. and Mrs. Maunder have considered the point, and are able to show from figures that the two phenomena are on very different scales.

Mr. Crommelin. In regard to what Dr. Lockyer has said about flocculi, I understand that these flocculi are perfectly invisible to the eye or to photographs, except those taken in the calcium rays. And then, if I am correct in that supposition, the flocculi could not act as a screen so as to obscure part of a spot. When flocculi cover the umbræ of spots, they (the flocculi) are utterly invisible in ordinary photographs, but at the same time the systematic predominance of the flocculi on the one side of the spot would indicate some want of symmetry in the Sun's surface. They might be an indication to some elevation which would act as a screen even if they did not act as a screen themselves. I suppose that is what Dr. Lockyer's words imply, not that flocculi themselves would act as a screen.

Dr. Lockyer showed a spectroheliograph picture of the large spot then on the Sun. It happened that this spot was nearly central in a mass of flocculous matter.

The President. I think I may offer the hearty thanks of the Meeting to Mrs. Maunder. The difficult matter in these researches is to break new ground, and I am sure you will all agree that Mrs. Maunder has broken very interesting ground in dealing with this subject. I would ask her whether she has any reply to make to the point raised by Prof. Turner, and I am sure we should be glad if Mrs. Maunder would speak through her own mouth instead of through the capable mouth-piece of her knight.

Mrs. Maunder. In regard to Prof. Turner's remarks, there is one great difference between the life-history of a man and of a sun-spot. A man may die suddenly, when at his full height and strength, as Prof. Turner has said; but, except in very rare cases, a sun-spot does not behave like that. I do not think that I know of an instance where a great spot has gone out suddenly and quickly. It may be born as a great spot, but it does not die as one, and so it is always the case of a small spot dying. The idea my enquiry has rather presented to my mind is that the Earth has an influence which tends to diminish the areas of spots, and so, at the same time, the length of life of the spot-group is shortened. That is to say that a spot which, if it were on the other side of the Sun, might perhaps have lived for nine days, on this side of the Sun might find its life cut short to perhaps eight days, so that there is a heavier death-rate on this side of the Sun than on the other. If there was no influence of the Earth, real or apparent, the birth-rate and the death-rate ought, on the whole, to be the same on this side of the Sun and on the other, but as it is we observe a very marked difference. As regards the possible veiling of spots by flocculi or faculæ, I hope before long to examine the question for faculæ as given in the Greenwich Daily Results. Of course, in that case, I shall hardly be able to treat the faculæ lune by lune, but must simply compare them as east or west of the central meridian, because the faculæ are not, as a rule, seen or measurable near the centre of the Sun. If this observed want of symmetry in spot-groups is apparent only and due to the heaping up of faculæ behind the spots, we shall probably get the same amount of faculæ on the east as on the west. But if it be due to some real influence exerted by the Earth, then we may expect to find a want of symmetry, though not necessarily of the same kind, in the faculæ also.

Mr. Reynolds (being called upon by the President to give a description of the photographic telescope that he has presented to the Helwan Observatory). The 30-inch mirror is one of those made by the late Dr. Common, and the flat mirror is by him also. The reason the instrument has been mounted at Helwan, near Cairo, is because I thought it would be far better to mount a powerful reflector in a good climate rather than in a climate such as we suffer from here. The construction was begun in June 1904, and most of the instrument was completed and erected in the autumn of 1905, but it was not until the beginning of this year that we really began photographic work with the instrument.

The photograph now on the screen shows the design of the instrument. I should say that the dome in which it is erected was originally intended for an 8-inch reflector which the Egyptian Government had set up, but which was much larger than was necessary for that instrument, and therefore it has been utilized for this reflector. The plan of the mounting is simple. First of all there is a concrete base, on that there is a cast-iron base 7 feet high in two sections, and then on the top of this comes the angle-block, which gives the adjustment for latitude.

Mr. Reynolds continued to show pictures of details of the mounting of the telescope, and added:—This has been found on the whole a good way of mounting a reflector; and although there are still some small items to be attended to, for everything is not perfect yet, we have found already that it is capable of very good results, and we have already started taking photographs with it. We took the first photograph in February of this year after a good deal of difficulty in getting adjustment. I might explain that there is much more difficulty in getting things done in a place like Egypt than there would be in England, and a great many make-shifts have to be invented on the spot; in this way Mr. Keeling has been most resourceful.

We had great difficulty in getting the cast-iron base up to the Observatory. The situation is 12 miles south of Cairo on the east side of the Nile on some hills. I have a slide here showing the base being brought up the hill.

Mr. Keeling. Helwan is the youngest Observatory in the world, and I am certain that, as regards work, I am the youngest astronomer, for the whole of my astronomical experience is comprised in the five pictures that I am going to show you.

[Photographs of the Orion nebula, of a star-cluster and three southern nebulæ were thrown on the screen and described by Mr. Keeling, and some questions of details were asked.]

The President. This is pleasant evidence of the rapid rise of a young astronomer. I am sure we hope that his decline will be very slow.

Prof. Turner. I came here prepared, if the photographs were not so good as they might be, to say a few words on behalf of those working at the Observatory at Helwan, but I am sure not a word is necessary. There is not a man here who would not be proud to exhibit as his first five photographs those that have just been shown.

Dr. Michie-Smith gave an account of recent work at the Kodaikanal Observatory, mentioning the exceptional number of fine days, more than 300, in the year, and showed photographs of the Sun taken with the spectroheliograph. He also spoke in appreciation of Mr. Evershed's help, and explained some ingenious devices invented by him.

The President. I am sure we can all understand the ring of satisfaction and achievement in the tone of Dr. Michie-Smith in regard to this most interesting field of work, and we may expect great things from the combined work of Mr. Evershed and himself.

Prof. Turner. I would like to add my appreciation of the fact that Dr. Michie-Smith has come nearly to the end of his main troubles. For many years he has been working practically alone and under many discouragements in India. But at last he has got efficient help; and we who know Mr. Evershed can well understand how glad Dr. Michie-Smith is to have him as a colleague. But my thoughts are rather now with the man who has been toiling for so many years without help, and with the satisfaction, that you Sir remarked upon, that he feels that he is at last able to see the beginning of a fruitful harvest.

Prof. Fowler. I should like to express my admiration for the splendid work that is being done at Kodaikanal. Several questions of interest are suggested by the beautiful photographs which Dr. Michie-Smith has shown and described, and I may perhaps be permitted to make one or two enquiries. Towards the end we had some very promising photographs of the spectra of sun-spots, and I noticed that one of them extended into the ultra-violet; according to Prof. Hale the characteristic phenomena seem to disappear in this part of the spectrum, and I should like to ask if this is confirmed by the Kodaikanal photographs, as it is a point not easily explained. Another interesting series of observations to which Dr. Michie-Smith referred was the comparison of prominences in calcium and hydrogen light; some of them appear to extend to a great height in the photographs, while in visual observations on the hydrogen line they were low. It would be interesting to know if such prominences were of the metallic class, because one often observes metallic eruptions where there is only a very small prominence visible, or practically no prominence at all, and it may be that these are connected with long extensions of the calcium column recorded by the spectroheliograph.

Dr. Lockyer. Dr. Michie-Smith did not mention it, but he photographed the prominences at the time he photographed the disk, which, I think, speaks well for the clearness of the air. In nearly every photograph the outline of the prominences can be traced. This is interesting, because he only took four minutes to photograph the limb, whereas we take a minimum of ten minutes. That gives a good idea of the difference of the air.

Sir David Gill. Why are not achromatic lenses used in the structure of the spectroscope?

The President. I believe I am responsible for that choice, which was made because a great length of slit was to be used. The image thrown on the slit is at least two inches. Now the collimator-lens and camera-lens, about which the question arises, are 4-inch lenses, and 2 inches is a big field to cover in such cases—at least, that is, to cover distinctly; but the field you can cover with a simple plano-convex lens is larger and flatter than that you can get with most object-glasses which have been corrected for achromatism. The question of difficulties arising was taken into consideration and was put on one side because the instrument was to be used for monochromatic light alone, so that the choice was dictated solely by the desire to get a large field with a small aperture. I was able to test the thing experimentally, and definitely to try what combination of lenses would give the best definition over a large plate with monochromatic light, and the result of those experiments, without any doubt whatever, was in favour of the simple plano-convex lens.

Prof. Turner. If I understand the case rightly, it was contemplated that the instrument would not be used for different colours, so that you had only to determine the focal length corresponding to the colour you wanted and to make the instrument in regard to that.

Sir David Gill. It is a new optical idea to me.

The President. I think what I have suggested is the result of experience.

Sir David Gill. It strikes me as peculiar, that is all.

The President. If the use of these simple lenses is causing any difficulty, it would not be a difficult matter to replace them by others. But I think we have evidence in the beauty of the photographs which Prof. Michie-Smith has shown us that the optical performance is pretty good.

Prof. Michie-Smith then read a paper by *Mr. Evershed* on the Prominences of the year 1906. It appeared that the distribution-curves for the first half of the year had the same form as that of the previous year, with a maximum in the zone 25°–30°, and a well-defined zone of activity in high latitudes, and on both sides of the equator there was a very pronounced minimum of activity in the zones 55°–65°. In the second half of the year the curve took a very different form, and there was a great falling off in activity in both hemispheres. The year as a whole is interesting as showing the culminating point in the prominence period, when the high-latitude zones of activity finally envelope the poles. This took place last at about the epoch 1894·7. A feature to be remarked about these high latitude prominences is the narrow limits of latitude within which at any one time the zone is defined, and the tendency to form extended chains of prominences all approximately in the same latitude, but often exceeding 100° in longitude. When this occurs one may get an apparently stationary prominence remaining at the same position-angle for 10 or 14 days.

Prof. Michie-Smith had pointed out, in showing the photographs of the Sun taken with the spectroheliograph, that frequently there appeared a dark line among the granulations on the solar surface that led up to a prominence on the limb. He suggested that these markings might be photographs taken in calcium light of prominences on the disk of the Sun.

Thanks were accorded to Prof. Michie-Smith and to Mr. Evershed.

The following papers were announced and partly read:—

J. Lunt. "On the Presence of Tin in Stellar Atmospheres."

F. Robbins. "Tables to accompany Mr. Innes's Paper on Computation of Secular Perturbations."

A. Stanley Williams. "On the Variable Stars RV and RX Andromedæ."

A. M. W. Downing. "Note on Le Verrier's Tables of Saturn."

H. H. Turner. "Note on the Range in Brightness at Maximum of Long-Period Variables."

Mrs. A. S. D. Maunder. "An Apparent Influence of the Earth on the Numbers and Areas of Sun-spots in the Cycle 1889–1901."

T. Lewis and H. H. Turner. "On the Inclination of Binary-Star Orbits to the Galaxy."

John Evershed. "Distribution of Prominences in Latitude in the Year 1906."

J. H. Reynolds. "Description of the 30-inch Reflector recently erected at the Helwan Observatory, Egypt."

H. F. Newall. "Note on the Spectrum of α Orionis."

Royal Observatory, Greenwich. "Observations of Jupiter's 6th and 7th Satellites from Photographs taken with the 30-inch Reflector."

The following Candidates were proposed for election as Fellows of the Society:—

Harold A. H. Christie, B.A., Flamsteed House, Greenwich (proposed by Sir W. H. M. Christie).

B. F. E. Keeling, Observatory, Helwan, near Cairo, Egypt (proposed by H. H. Turner).

F. A. Lindemann, Sidholme, Sidmouth, Devon (proposed by A. F. Lindemann).

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

SION COLLEGE, VICTORIA EMBANKMENT.

Wednesday, 1907 April 24.

F. W. LEVANDER, *President*, in the Chair.

Secretaries: J. G. PETRIE.
H. P. HOLLIS (*vice* J. A. HARDCASTLE).

THE Minutes of the previous Meeting were read and confirmed.

The names of four Candidates for Membership were read and passed for suspension, and the election by the Council of one new Member was unanimously confirmed.

The list of presents received was read, and the thanks of the Association were accorded to the respective donors.

The President said he regretted to have to announce that Mr. Hardcastle, one of the Secretaries, was laid aside by a serious illness. During his absence Mr. Hollis had kindly undertaken to discharge his duties.

Mr. Hollis read a note by *Mr. W. F. Denning* on "June Meteors." The writer observed that June was a very unsuitable month for meteoric observation in our latitude in consequence of the prevalence of twilight, but the beautiful weather rendered night work very pleasant, and if no special showers marked the month, there were a number of interesting minor streams visible. The writer then gave the positions of a number of streams observed by himself since 1877 which it would be interesting to re-observe. Some of the radiants indicated might have become practically extinct, while others might have come strongly into play.

Mr. Maunder read a letter which he had received from a stranger, not a Member of the Association, describing a special kind of crepuscular rays that he had noticed. Faint bands, *white* in colour, diverging from a spot opposite the Sun. The longest band or ray reached as far as 20° from the horizon. They appeared to be symmetrically placed exactly opposite the Sun. They differed from similar phenomena as ordinarily described in that (1) they were without any pink colour, (2) they appeared *before* sunset and disappeared *at or before* sunset, (3) that no corresponding rays were visible in the west.

Mr. Newbegin said the description contained in the note read by Mr. Maunder corresponded with a phenomenon his son and himself saw while out walking one afternoon. In the sky exactly opposite to the Sun were similar rays to those that had been described, and they were very much puzzled to account for their appearance, springing, as they did, from the horizon and spreading upwards.

Mr. Newbegin then read a paper on "The Solar Prominences of 1906." He had observed 381 prominences—229 bright and 152 faint,—and but for an unavoidable stoppage in the middle of summer, he believed the list would have been an exceptionally long and interesting one. On twelve occasions black matter was observed. Capt. Daunt, a very assiduous observer, had also noticed black matter. It appeared that this black matter was more easily seen with a grating than with a prism. A number of slides were then shown in illustration of the paper.

Mr. H. Ellis asked if Mr. Newbegin observed the black matter in other than hydrogen light, whether the matter was really black or some other colour that appeared black in the red light.

Mr. H. Keatley Moore asked what reason there was for calling the black appearance "black *matter*." Why should it not be simply a void?

Mr. Newbegin said the question just put was a very reasonable one. He was greatly in doubt when he first observed the black material, and to prove its existence he rolled the grating over into the third order, where, instead of a dark, diffused light in the slit, there was a green background and a green ground generally. The C line was green in the third order, and the prominence showed itself red, with sometimes a black bar actually crossing it, and sometimes a black dot at its base.

Father Cortie said:—Mr. Newbegin's reply deals with the question of the background only. It would have been more satisfactory had the observations been made on some other prominence lines, as D_3 or F, in addition to the C line. Trouvelot, years ago, observed such black prominences, and there was no doubt about their objective reality. At the same time it was very necessary to be careful to eliminate a mere contrast-effect. With regard to the mean daily number of prominences, the numbers given by different observers for the same years differed very considerably, so that to draw a curve of prominence frequency it was impossible to combine the observations of different observers. The Stonyhurst observations of prominences for a cycle of eleven years had been reduced in this manner, and the resulting curve was in close agreement with the curve of sun-spot disc-area obtained from the Greenwich observations over the same period.

The President said there were so few spectroscopic observers in England that they were very grateful to Mr. Newbegin as a private observer for bringing forward any remarks he had to make on his observations, and he (the President) was sure they would return him their hearty thanks.

Father Cortie then delivered a lecture on "Recent Spectroscopy at the Stonyhurst Observatory." A large number of photographs of the instruments employed at Stonyhurst, and illustrating the work done there, were thrown upon the screen. The lecturer commenced by giving a short history of the science of spectroscopy from the discoveries of Newton to the present time, and then proceeded to describe in detail the various instruments employed at the Observatory. The lecturer pointed out that for the production of a bright line on a continuous background it was not necessary, as was so often stated, that the glowing absorbing-vapour should be at a higher temperature than the source of the continuous spectrum, and also that the absence of the lines of an element from the spectrum of the Sun or a star was no absolute criterion of the absence of the substance. Various types of spectra, especially the gradual transition of one type into the other, the flutings being gradually replaced by lines, were illustrated by a beautiful series of spectra taken by Father Sidgreaves. The double spectra of short-period variables was also illustrated by a series of photographs of the spectrum of β Lyræ, showing the alternations in the positions of the bright and dark lines at every phase of the star's variations in brightness. In the case of β Aurigæ the doubling of the K line indicated when the two components were moving in opposite directions in the line of sight. Finally, the spectra of new stars was illustrated by examples of the spectra of Nova Persei and Nova Aurigæ.

Mr. Newbegin said he did not think there were any questions to put to Father Cortie, but he had listened with interest and delight to the lecture from beginning to end.

The President said it was extremely kind of Father Cortie to come there and give them such an interesting lecture. Beginning at the first elements, Father Cortie had carried them on in the most lucid and fascinating manner until he had brought them to some of the latest results arrived at at Stonyhurst. He hoped that the result of the lecture would be to bring some additional Members to their Solar Section.

The President then announced that Mr. Rendell, late of the Natal Observatory, and formerly of the Royal Observatory, Greenwich, was present, and had some slides of the Natal establishment, which he would be pleased to show and describe.

Mr. R. Fermor Rendell said he thought it might be of interest to the Association to see a few photographs of one of the minor observatories. A large part of the time of the staff was taken up with meteorological work. Records of rainfall, temperature, pressure, &c., extending over twenty years, had been kept. One of the difficulties they had to encounter was the damage done by white ants, which not only destroyed papers, sometimes of great value, but even the woodwork of the Observatory itself had frequently to be renewed.

The President, in the name of the Association, thanked Mr. Rendell for kindly showing the photographs of the Natal Observatory, which would be remembered as the place where Mr. Nevill (who had, under his former name of Neison, published a very valuable book on the Moon) laboured so successfully.

Mr. Maunder, in answer to the President, said there was not much to say about the new comet, except that it was a faint object moving very rapidly. He was afraid it was beyond the reach of any of the telescopes in the possession of Members of the Cometary Section. Had it not been so, he would have sent a notice to them on the subject.

Mr. Maunder then read an extract from a well-known weekly paper on "Will the World Die by Fire?" in which it was gravely stated that the Sun with the Earth and other planets oscillated between Arcturus and Polaris, being burnt up whenever it got near Arcturus, and frozen whenever it reached Polaris.

The Meeting then adjourned at 7 P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE first of the afternoon Meetings for the present Session was held on Wednesday, May 15, at the Society's Rooms, 70 Victoria Street, Westminster, *Dr. H. R. Mill*, President, in the Chair.

Dr. Mill read a paper on "The Standard Rain-Gauge, with Notes on other Forms." When the late Mr. Symons founded the British Rainfall Organization forty-seven years ago, such observations as were being carried on were made with rain-gauges of the most varied patterns, set up at any height from the ground that suggested itself to the observer, and read irregularly at almost any hour of the day or night. Since that time there has been a steady approximation to uniformity, and now the greater number of rain-gauges in use are of a few definite patterns, set for the most part at nearly the same height above the ground. Dr. Mill strongly recommends the Snowdon pattern rain-gauge, which is 5 inches in diameter, has a vertical rim to the funnel of 4 inches, and has an inner can and also a bottle. He does not recommend rain-gauges with shallow funnels, nor the Howard and Glaisher patterns.

Col. J. E. Capper gave an account of a captive balloon being struck by lightning at Farnborough during a thunderstorm on April 11. The lightning-flash appeared to travel along the wire until it reached the wagon, then a sudden bright light appeared and ran right up the wire into the clouds in which the balloon was hidden. The wire was fused, being burnt entirely away where it first touched the iron pulleys which guide the wire when running out. One side of the balloon and net was burnt, owing to the hydrogen catching fire, but the other side was uninjured.

Prof. A. Herschel gave an account of a remarkable excavation made by lightning in peat-earth on August 2 or 3, 1906, in a moorland district of Northumberland.

Mr. J. W. Lovibond exhibited and described his apparatus for measuring fog-densities.

The Sun-spots of 1907 *February.*

It is remarkable how often the month of February has been distinguished in late years by extraordinary outbursts on the surface of the Sun. Thus in February 1892 the largest group of spots observed for twenty years attained its greatest development. In February 1894 another giant formation, not much inferior to that of 1892, passed through a striking series of changes. Both these disturbances synchronized with great magnetic storms on the Earth. Then in February 1905, the great spot of thirteen years earlier was surpassed by one more gigantic still, and that has now been followed, in February 1907, by a group not much inferior in total area, and, being less compact, exceeding it in its extreme extent.

February opened with seven groups bequeathed to it by January, two of these, Groups 6099 and 6100, being visible to the naked eye. Then, on February 4, the region of the great January disturbance began to come into view at the east limb, this time even more violently perturbed than before, and when it had nearly reached the centre of the disc, on February 10 and 11, no fewer than four groups were separately visible to the naked eye. One of the four was an important stream which had newly broken out on the visible disc in advance of the great disturbance; the other three were contributed by the chief disturbance itself. Its main component was Group 6108, a development of Group 6081, and by February 10 a magnificent stream, which attained its greatest area on February 13, when it nearly equalled the giant group of 1892.

Group 6108 passed off at the west limb on February 19, and on the same day a fine stream, Group 6118, likewise in the southern hemisphere, appeared at the east limb. This group was also easily visible to the naked eye, and lasted until the end of the month. A number of small groups broke out, or appeared at the east limb on the last few days of the month, which closed with the Sun's disc marked by ten or eleven widely scattered centres of activity. Besides the two great disturbances, Groups 6108 and 6118, the record of the month included several streams of only less importance, and on the whole 1907 February stands out as the most prolific month as to the numbers and areas of spots since 1905 October.

Rotation 714 began 1907 February $8^{d}\cdot316$.

Notes on the Principal Spot-Groups.

Group 6103.—Return of Group 6075, or a revival of activity in its neighbourhood. A fine large composite spot, followed by a train of small unstable spots. Area on February 10 about 800.

Spot-Groups in 1907 February.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1907.	1907.	days.	d	°	°	
6103	Feb. 4	Feb. 16	13	Feb. 10·1	337	−13	**Return.** Fine composite spot.
4	5	13	9	7·3	13	−15	Irregular stream.
5	5	11	7	10·7	328	−17	**Return.** Few small spots.
6	5	7	3	11·0	325	−13	Few small spots.
7	5	16	12	11·1	323	− 4	Large regular spot.
8	6	19	14	12·4	306	−17	**Return.** Superb stream.
9	8	13	6	9·2	349	− 6	Stream *np*. Gr. 6103.
6110	8	11	4	11·7	315	+15	Few very small spots.
1	10	16	7	10·1	337	+15	Stream of small spots.
2	12	16	5	17·9	234	−18	**Return.** Small spot.
3	13	25	13	18·9	220	+16	**Return.** Straight stream.
4	14	15	2	11·1	323	+20	Very small spot.
5	16	21	6	22·4	175	+ 6	**Return.** Small spot.
6	18	21	4	23·1	166	− 8	One or two small spots.
7	18	Mar. 1	12	23·2	164	+ 4	**Return.** Regular spot.
8	19	1	11	22·7	171	−13	Straight stream.
9	20	Feb. 27	8	22·2	177	+10	Straight stream.
6120	21	25	5	26·7	118	+16	Few small spots.
1	21	Mar. 2	10	27·2	111	− 8	Regular spot.
2	23	Feb. 24	2	24·6	145	− 8	Small irregular stream.
3	24	Mar. 7	12	Mar. 1·9	76	+24	**Return.** Regular spot.
4	25	Feb. 27	3	Feb. 27·0	114	+ 5	Small short stream.
5	26	Mar. 1	4	Mar. 4·0	48	+14	**Return.** Few small spots.
6	27	Feb. 28	2	Feb. 25·9	128	− 5	Very small spot.
7	27	Mar. 6	8	Mar. 5·2	32	+ 6	Regular spot.
8	28	4	5	Feb. 28·5	94	+10	Compact cluster.
9	28	7	8	Mar. 3·2	59	−15	Long straight stream.
6130	28	1	2	5·7	26	−12	Small faint spot.
1	28	9	10	5·8	24	+10	Large regular spot.

Group 6104.—An irregular stream, breaking out *p*. Group 6103. Area on February 8 about 750.

Group 6105.—With Group 6106 a return of Group 6076. A number of small unstable spots *sf*. Group 6103. Area on February 9 about 20.

Group 6107.—A large regular spot. Area on February 9 about 200.

Group 6108.—Return of Group 6081. A magnificent stream. Area on February 13 about 2700; length more than 16°, breadth 8°.

Group 6109.—A stream forming *np*. Group 6103. Area on February 10 about 120.

Group 6111.—A stream of small spots. Area on February 13 about 200.

Group 6112.—Return of Group 6088. A small regular spot. Area on February 15 about 10.

Group 6113.—Return or rather revival of Group 6087. A number of spots in a straight stream. The group broke up into two compact clusters before February 20. Area on February 18 about 400.

Group 6115.—Return of Group 6090. Third and final apparition. A small spot. Area on February 18 about 20.

Group 6117.—Return of Group 6093. A large regular spot with a few small companions. Area on February 23 about 350.

Group 6118.—A straight stream rapidly increasing in size. Area on February 24 about 1200.

Group 6119.—A number of small unstable spots in a straight stream *nf.* Group 6115. Area on February 23 about 350.

Group 6121.—A regular spot followed at first by a short train of small spots. Area on February 26 about 100.

Group 6123.—Return of Group 6099. A large regular spot. Area on February 27 about 300.

Group 6125.—Return or revival of Group 6101. A few small scattered spots. Area on February 28 about 25.

Group 6129.—A number of small spots in a long straight stream. Area on March 2 about 170.

Group 6131.—A large regular spot. Area on March 4 about 180.

E. WALTER MAUNDER.

The Meeting of the International Solar Union.

It will be seen on other pages of this number that a very successful Conference was held at Meudon between May 20 and 23. By the kindness of one who was present we are able to give a short summary of the formal proceedings, and the following list of names of those who attended (which may not be perfectly complete) will show the representative character of the gathering:—

FRANCE (43).

Andre.
Angot.
Baillaud.
Baillaud (*fils*).
Bassot.
Becquerel.
Benoit.
Bigourdan.
Bosler.
Bouquet de la Grye.
Bourget.
Darboux.
Deslandres.
Fabry.
Féry.
Flammarion.
Gonnessiat.

Gramont, Count de.
Guillaume (Lyons).
Guillaume (Bretueil).
Hamy.
Janssen.
Kenapel.
Lebeuf.
Lippmann.
Lœwy.
Marchand.
Mascart.
Millochau.
Moureau, l'Abbé.
Nordmann.
Perot.
Picard.
Pluvinel, Count de la Baume.
Poincaré.
Pringsheim.
Puiseux.
Salet.
Stephan.
Stefanik.
Trépied.
Teisserenc de Bort.
Violle.

America (2).

Ames.
Hale.

Austria (1).

Weiss.

Germany (3).

Hartmann.
Kayser.
Kempf.

Great Britain (12).

Cortie.
Duffield.
Fowler.
Hinks.
Lockyer, Sir J. N.
Michie-Smith.
Newall.
Sampson.
Schuster.
Shaw.
Turner.
Wilson.

Holland (1).

Julius.

Italy (2).

Chistoni.
Rizzo.

Norway (1).

Birkeland.

Russia (4).

Belopolsky.
Donitsch.
Hansky.
Stanœvitch.

Spain (1).

Cirera.

Switzerland (1).

Wolfer.

Monday, May 20. M. Lippmann in the Chair.

Opening business, after which *Count de la Baume Pluvinel* proposed the formation of a Committee to be charged with formulating a uniform plan of publishing eclipse observations.

Mr. H. F. Newall proposed, *Prof. Hale* supporting, that action be taken in the matter of a decision arrived at on the motion of Prof. Campbell at the Oxford Conference, that the problem of investigation of solar rotation by spectroscopic methods be included among those to be dealt with by the Union. He proposed the formation of a Committee to put forward a plan of co-operation among observers.

Prof. Fowler proposed the formation of a Committee to organize visual observations of prominences.

Prof. Sampson spoke of the desirability of not excluding from the sphere of work of the Union co-operation among those working at the theoretical aspects of Solar Physics.

Tuesday, May 21. M. DESLANDRES in the Chair.

Prof. Kayser presented the Report of the Committee on Standard Wave-lengths.

MM. Pérot and Fabry announced the completion of their redetermination of the wave-length of the red cadmium line, which showed complete agreement with the result found by Prof. Michelson, and described the progress in the work of determining secondary standards.

Prof. Fowler presented the Report of the Committee on Sunspot Spectra, and announced Prof. Young's resignation of the office of Chairman.

Prof. Hale showed a preliminary photographic map of sun-spot spectra from λ 4600 to 7200.

Wednesday, May 22. M. BECQUEREL in the Chair.

Prof. Schuster read the Report of the Committee on Solar Radiation, and announced Ångström's resignation of the Chairmanship on account of his health.

Prof. Hale gave a Report of the Committee on Spectroheliograph Observations, and showed some superb photographs taken at Mount Wilson. He called attention to the need for the Congress to consider methods of measurement and discussion of photographs of flocculi, and described an instrument by himself which afforded one method of dealing with the measurement.

The afternoon of this day was spent at the Physical Laboratory of the Sorbonne, where *Prof. Julius* described his observations on anomalous dispersion, and demonstrated the phenomena arising from varied temperature-gradients in sodium vapours. *M. Lippmann* showed coloured photographs. A visit was also made to the Paris Observatory, at the invitation of M. Lœwy, where the new star-spectrograph attached to the Coudé Equatorial was inspected, and M. Hamy's interferometer experiments.

Thursday, May 23. M. VIOLLE in the Chair.

M. Belopolsky spoke of his observations of the K line on the solar surface.

Prof. H. H. Turner presented the Report of the Central Computing Bureau, and reported the work done in relation to the redetermination of the Sun's axis of rotation.

The Conference was closed by the appointment of Committees, and the fixing of the date of the next Conference, which is to be in California in 1910.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Glasgow Professors of Astronomy.

GENTLEMEN,—

Many of us retain a pleasant and kindly recollection of the last Professor of Astronomy at Glasgow, Robert Grant, who spent some time at Greenwich in acquiring a knowledge of the practical part of the science before taking up his appointment. His 'History of Physical Astronomy' was published in 1852. For it he obtained the Gold Medal of the Royal Astronomical Society in 1856, and it is one of those works of which it may safely be predicted that it will never become obsolete, dealing as it does with the period during which the foundations of modern astronomy were laid, and being very accurate and complete up to the date of its publication.

Grant died in 1892, in the seventy-ninth year of his age. He had been appointed to the professorship at Glasgow in 1859 *, almost exactly a century after that office was established. The first Professor was Alexander Wilson, a great friend of William Herschel, who was born at St. Andrew's in 1714 and received the appointment in 1760. From the first the professorship (which was called of Practical Astronomy until 1893) was provided with a small observatory in the college grounds. Wilson's name is universally known amongst astronomers in connexion with what is called the Wilson phenomenon of the solar spots, which so many books adduce as proving the cavernous nature of these formations, and that the greater part of them lie below the general surface of the photosphere. The writer of his life in the 'Dictionary of National Biography,' indeed, speaks of this as Wilson's "celebrated discovery." It was, however, contested from the first, and the persevering observations (extending over a period of thirty years) of the Rev. F. Howlett have shown that the appearance in question is certainly not the normal one, and made it probable that in most cases when the cavernous nature has been seen it is of a shallow kind, what is called the Wilson phenomenon being seldom of a marked character.

Wilson had his son Patrick appointed his assistant in 1782, and the latter succeeded him on his resignation in 1784; two years afterwards Alexander Wilson died. The son continued to hold the appointment until his own death in 1799, when he bequeathed to the University the sum of £1000 on condition that the interest therefrom should be applied to the purchase of instruments and books for the use of the Professor and his students.

Prof. Meikleham succeeded Patrick Wilson in 1799, and James

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* In the following year he joined the Himalaya expedition to Spain, and observed the total eclipse of the Sun, in July 1860, at Vittoria.

Cooper followed in 1803. The latter held it for thirty-three years and was succeeded by John Pringle Nichol in 1836, who was the immediate predecessor of Robert Grant. It was under Nichol that the observatory was removed in 1840 from the college grounds to its present situation on Dowanhill, and he made an expedition about the same time to Munich in order to provide the new establishment with some suitable instruments. His books are well known. Of these, Miss Clerke remarks, in the 'Dictionary of National Biography,' that they were, "like his discourses, eloquent, enthusiastic, and learned." She tells us that "George Eliot," amongst others, greatly admired them, and they did much for the popularisation of science. In one, called 'The Planetary Systems,' Nichol made the earliest suggestion for the study of the solar spots by photography. His last and greatest work was his 'Cyclopædia of Natural Sciences,' and he was engaged on the preparation of a second edition of this when he died in 1859, in the fifty-sixth year of his age. But it is of his 'Architecture of the Heavens,' which appeared in 1838 and ran through ten editions, that I wish now particularly to say a few words. Although, of course, our increased knowledge of stellar astronomy since the author's time has altered our views of the universe (shall we say universes?), he had assimilated the great discoveries of Herschel and Lord Rosse (the ninth edition, which appeared in 1851, is dedicated to Lady Rosse as a mark of respect to her illustrious husband, who had received the Royal Medal in that year), and even now one cannot read the work without interest, written as it is in a great spirit of reverence. But it is to one point only that I desire at present to direct attention.

Several efforts have been made, commencing with those of W. Herschel and Prevost, to ascertain approximately the region in the heavens towards which our own Sun and system are moving. Nearly all indicate one in or near the constellation Hercules; and I suppose even the recent investigations of Prof. Kapteyn and Mr. Eddington with regard to great universes moving in different directions cannot affect this as a *relative* motion, any more than if I had been in the habit of travelling from London to York before being acquainted with the Earth's axial and orbital motions, I should change the direction of my relative motion when I knew how different it was from my actual motion in space. One would like, however, to see these investigations applied to a much larger number of stars and in different parts of the stellar heavens. Also it seems to me that an effort might be made, remembering that what we call proper motions of the stars are only in fact the tangential parts of them, by combining these with motions of approach and recess determined by the spectroscope, to deal with true motions in space before deducing solar motion or star-drift from them.

When we discuss the probable direction of the motion of the Sun in space, we are inevitably led on to the further speculation of what centre it may be revolving round in its gigantic orbit. Three positions of this have been suggested. Mädler located it in the Pleiades, but this idea has met with little acceptance; as Sir John Herschel says, it is almost inconceivable that any general circulation can take place so far from the plane of the Milky Way. Mr. Maxwell Hall suggested the region of the constellation Pisces; but though his method is the true logical one, founded on actual parallaxes as well as proper motions, it can only be applied in our present state of knowledge to a very few stars, and therefore can hardly claim much weight. Thirdly, we have the suggestion of Prof. Nichol, which I will give in his own words from p. 261 of the 'Architecture of the Heavens':—"Supposing a great plane to be drawn through the Sun at right angles to the line between him and the foregoing point in Hercules, the centre of the orbit must be in or near that plane; and if other considerations could indicate another plane in which also the centre must lie, then the intersection of the two would determine approximately the position of that centre." Then he takes this other plane to be that of the Milky Way, and finds the intersection in the constellation Perseus, a very rich region, in which he finally proposes to locate the centre of the solar motion.

Yours faithfully,

Blackheath, 1907, May 8. W. T. LYNN.

The Comet of 1006.

GENTLEMEN,—

'Ali ben Ridwân (Rodwân), about whom Mr. Lynn enquires in your last number, was a native of Gizeh, near Cairo. At the age of fourteen he began to study Philosophy and Medicine, but it was not until he had reached his thirty-second year that his name as an able physician was assured and he entered the service of the Caliph El Hakim. He died at Cairo in the year 453 of the Hijra, or A.D. 1061. As El Hakim was murdered in 411 or A.D. 1020, Ali must have been born at the latest in 989, so that he could very well say that he was a youth when he saw the comet of 1006. The mistake of referring this comet to the year 1200 was made by Riccioli, whom Hevelius, Lubienitzky, and Struyck have followed; it must have been caused by an error of translation in the Latin edition of 'Ali ben Ridwân's commentary to the 'Quadripartitum' of Ptolemy.

According to Ali this comet appeared in the middle of the sign of Scorpio (15th degree); the head was three times as large as Venus and it gave as much light as a fourth part of the Moon. It moved from east to west as far as the 15th degree of Virgo. It must have appeared about the 30th April, which is the date calculated by Pingré from the places of the planets given by Ali, probably from calculation. It is very remarkable that a star as bright as Venus is stated by several chronicles to have appeared at that time. The Annals of St. Gall say that it was seen for three months "in intimis finibus austri ultra omnia signa quae videntur in coelo." The Syrian Chronicle of Abu'l Faraj places it in the zodiacal constellation of 'akraba or Scorpio; its rays were "moved," it shone like the Moon and disappeared after lasting four months. According to the Arabian Chronicle of Ibn al Athîr a great star like Venus appeared on the 3rd May, 1006, to the left of the direction of Mecca from Babylonia; it shed its rays on the Earth like the rays of the Moon and lasted till the 13th August. See Schönfeld's paper on this star, Astr. Nachr. 3034 *.

It is a pity that Abu'l Faraj does not mention this star in his excellent work on Ptolemaic astronomy, but after all we could hardly expect him to include phenomena of this kind in a solemn treatise on mathematical astronomy. Chinese observers do not refer to a comet in the year 1006, but the Ma-Twan-Lin has an "extraordinary star" in the south-east under the date April 3, 1006. See Biot, Conn. des T. 1846, p. 67.

The Chronicles of the Ma-Twan-Lin do not mention any motion of the star. But the words of the 'Annales Sangallenses,' "Quae mirum in modum aliquando contractior, aliquando diffusior etiam extinguebatur interdum," can hardly refer to a fixed star. And as date, brightness, and constellation all agree, it seems to me very probable that there was not any temporary star in 1006, but only a comet.

Yours faithfully,

J. L. E. Dreyer.

Armagh Observatory,
1907, May 11.

D_3 *Absorption in the Solar Spectrum.*

Gentlemen,—

Mr. Nagaraja, in his letter in your last May number, has presented the matter to me in a somewhat new aspect, and on reading his letter I made an examination of my observations for this year.

Like Mr. Buss, I have never seen D_3 in absorption over the umbra of a spot, but only in the vicinity of an active group; almost invariably in rear of the leader of a group.

I am unable to refer to my observations for last year as they are with Father Cortie, but I am under the impression I have recorded observations of D_3 dark over faculæ where no spots are present.

So far this year I have recorded no observation to that effect, but I have observed D_3 dark over the faculæ in front of a group when nearing the western limb.

Like others also I have found the appearance of the C line an almost infallible guide to the appearance of dark D_3. I have, of course, many observations of a disturbance of the C line when D_3 dark was not seen, but the appearance of D_3 is frequently very intermittent—it will be seen at, say, 10 A.M. and not at 1 P.M., though the C line may be agitated at both hours.

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* The comet is not alluded to in this paper.

On the other hand, I have never seen the absorption line of D_3 unless the C line showed disturbance.

Now hitherto I had not regarded the particular form the disturbance of C took. I regarded distortions, notches, reversals, a lumpy and twisted appearance indifferently. Mr. Nagaraja draws particular attention to reversals, a point of view which is new to me.

In all I have recorded 53 observations of D_3 in absorption this year up to the present time. On four occasions only D_3 appeared dark when the C line was disturbed but not reversed; but, on the other hand, there were only five occasions in which I observed reversals of C without recording other disturbances of the line, such as distortions, notches, &c. I cannot, therefore, say that my observations, such as they are, show that reversals of C are the particular form of disturbances which favour the appearance of D_3 in absorption, but, on the other hand, there is nothing in my observations in disagreement with Mr. Nagaraja's contention.

Mr. Nagaraja states in his letter "bright D_3 on the disc of the Sun occurs, so far as my observations go, only in the umbra of spots; it abruptly stops short even in the penumbral spectrum." I have recorded observations of bright D_3 in the vicinity of spot-groups, but only on occasions of exceptional outbursts, notably in the great outburst of March 3rd last. This was, however, only a prominence seen in projection, which could be made out in bright C and bright D_3 on the disc.

Personally I am inclined to the belief that in the majority of cases reversals of C are merely exceptionally bright prominences seen in projection; these outbursts frequently accompanying active spot-groups.

I frequently compare notes with Mr. Buss and our observations agree very well.

In most cases I might mention, I see D_3 broken and patchy: broad, not a sharp line at all; sometimes as a string of black dots.

Yours faithfully,
R. DAUNT.

Lynatta, Newtownards,
Co. Down.
1907, May 15.

Saturn.

GENTLEMEN,—

With Saturn becoming more favourably visible in the morning sky, it is hoped observers will examine the planet for traces of spots and irregularities in the belts. The present time offers good opportunities for detecting any equatorial white spots that may be visible. It is important to determine the rotation period from further markings, the value found by Herschel from a quintuple belt in the southern hemisphere (in 1793–4) being

$10^h\ 16^m\ 0^s{\cdot}44$, by Prof. Hall from an equatorial white spot (in 1876-7) $10^h\ 14^m\ 23^s{\cdot}8$, and by the writer from a number of white and dark spots in N. temp. region (in 1903 July-December) $10^h\ 37^m\ 55^s{\cdot}2$. The value derived in 1903 exhibits a difference of more than 20 minutes as compared with the earlier determinations.

The disturbances visible in 1903 in the northern hemisphere appear to have been on a larger and more abundant scale than had ever previously been recognized. Since the year named there have been but slender and occasional evidences of irregularities in the dark belts or bright zones, but a few spots were seen in 1905 and 1906 in similar positions to those of 1903. I may quote a few of my observations with a 12½-inch reflector by Calver:—

	h m	
1905 July 6......	14 20	A small brilliant spot in N. temp. region between W. limb and C. M. of Saturn must have been on C. M. about $13^h\ 40^m$.
1905 July 13......	15 40	Small bright spot on C. M.
1906 Sept. 11......	10 20	Rather small bright spot followed by dusky patch in N. temp. belt, on E. side of C. M. Central about $10^h\ 45^m$.

The power used was about 310. My observations of the markings were not, however, sufficiently numerous to enable me to re-investigate the rotation period. On certain nights when the definition was remarkably good no spots could be discerned.

Further observations of the surface-markings of this beautiful planet would be interesting. Yours faithfully,

Bristol, 1907, May 2. W. F. Denning.

PUBLICATIONS.

Harvard Annals. — Two specially interesting parts of the Harvard Publications have lately been issued. One (vol. lii. part 1), "Eclipses of Jupiter's Satellites," contains the observations that Prof. Sampson, of Durham Observatory, is now discussing with the view of improving the theory of the satellites' motion, as he described at the Meeting of the Royal Astronomical Society in April last. This part does not contain Prof. Sampson's discussion, which is reserved for the second part of the volume, but it gives descriptions of the instrument and of the method of observation and reduction.

The other volume referred to is a very complete Catalogue of Variable Stars *, prepared by Miss Annie J. Cannon, the opening paragraph of which we take leave to quote:—"The history of variable star catalogues extends as far back as 1844, when there appeared in Schumacher's 'Jahrbuch,' p. 214, a catalogue of 18 variable stars compiled by Argelander. The following stars compose the list, which included all those far enough north to be

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* "Second Catalogue of Variable Stars," Harvard Annals, vol. lv. pt. 1.

visible from Germany and considered by Argelander to be certainly variable. They are arranged in the order in which they were discovered to be periodically variable. They are *o* Ceti, χ Cygni, R Hydræ, β Persei, η Aquilæ, β Lyræ, R Leonis, δ Cephei, *a* Herculis, R Coronæ, R Scuti, R Virginis, R Aquarii, S Serpentis, R Serpentis, *a* Cassiopeiæ, *a* Orionis, and *a* Hydræ. It is interesting to note that of these 18 stars *a* Hydræ is not now included among the variables, and there is still a difference of opinion regarding the reality of the variation of *a* Herculis, *a* Cassiopeiæ, and *a* Orionis." The subsequent catalogues of variable stars which are then referred to may be tabulated as follows:—

1854.	Pogson's Catalogue of 53 Variables.	Radcliffe Observations, vol. 15.
1864.	Chambers's „ 123 „	*Ast. Nach.* 63.
1865.	Schönfleld's „ 113 „	„ 64.
1875.	Schönfeld's 2nd „ 143 „	
1883–1888.	Schönfeld's Catalogue was periodically enlarged at Harvard Observatory, and publication was made at various times in the 'Proceedings of the American Academy.'	
1888.	Vol. xviii. No. 8 of 'Harvard Annals' was published, containing an Index to Observations of Variable Stars and a Catalogue of 225 objects.	
1888.	Chandler's 1st Catalogue of 225 Variables.	*A. J.* viii. p. 81.
1893.	„ 2nd „ 260 „	„ xiii. p. 89.
1896.	„ 3rd „ 393 „	„ xvi. p. 145.
1901.	Alex. Roberts's Catalogue of 94 Southern Variables.	*A. J.* xxi. p. 84.

An ephemeris of variable stars has been published annually in the 'Vierteljahrsschrift der Astronomischen Gesellschaft' since 1870. The catalogue for 1906 contains 709 stars.

In 1903 a Provisional Catalogue of Variables was published at Harvard containing 1227 stars, including variables in clusters. The present catalogue contains 1957 stars, counting those in globular clusters, but not those in the Magellanic clouds where 1791 have been found, so that there are now at least 3748 known variable stars. This interesting summary by Miss Cannon shows the immense increase in the number of known variables in the last half-century.

It is not necessary to describe the details of this work. The compilation has evidently been made with great care and from the best authorities, and results in a tabular catalogue of 1265 entries, with a supplement of 36 pages giving a history of nearly every star. If one wants to know the facts about the discovery of Nova Andromedæ, Nova Persei, or any other Nova or Variable, they will be found in these pages. There are some supplementary tables, which add to the completeness of the publication and make it a useful contribution to this branch of astronomy, for the publication of which the authoress and Harvard may well be thanked and congratulated.

'THE ZODIA; or, The Cherubim in the Bible and the Cherubim in the Sky.' *—It is now generally recognized that the constellations were devised some 5000 years ago by men who had a knowledge of astronomy, but who did not map out the stars into constellations for astronomical purposes. But the originators must have intended to signify something by these mysterious figures. What, then, is the origin of the constellations, and what is their significance? The reply given by the author of the book before us is that their origin is divine and that their significance is religious. The greater part of the book is accordingly devoted to the expounding of the religious signification of the constellations of the Zodiac. As this is an astronomical and not a religious publication, we cannot discuss this rather novel view of the zodiac, but some of the purely astronomical points mentioned by the author are worthy of attention.

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* By E. M. Smith. London: Eliot Stock, 1906. Price 6*s*.

Mr. Smith quotes Lord Grimthorpe's remark that "The connection of the vernal equinox with the constellation and the star Alcyone, η Tauri, is recognized by tradition all over the world." The vernal equinox occupied this position about 2200 B.C. A few centuries earlier, the equinoctial point was about the middle of the constellation Taurus. If we divide the ecliptic into four equal parts, beginning at the middle of Taurus, we find the other three points will fall about the middle of Leo, Scorpio, and Aquarius respectively. Mr. Smith accordingly divides the zodiacal constellations into four groups of three each, and considers the central figure of each group the most important of the three, the central figures being those of the Bull, the Lion, the Scorpion, and the "Man of Reuben." If the constellations were arranged when the equinox was in Taurus, we should have expected the first month of the year to be associated with this constellation. But we do not find this to be the case. The ancient Akkadian calendar, which was adopted by the Babylonians, Assyrians, and Jews, made Nisan the first month, and its associated constellation, Aries, the first sign of the zodiac. This peculiarity is noticed by Mr. Smith, but he gives no adequate astronomical explanation of it.

There are some interesting remarks on the history of the constellation Libra. If we look at the figure as depicted on a star-atlas we see that it is rather cramped for room, giving one the idea that it was inserted as an afterthought between Virgo and Scorpio. The name is comparatively modern. In Aratus it is called the "Claws." If, as Mr. Smith suggests, the "claws" belonged to the Scorpion, there must have been only eleven zodiacal constellations in the time of Aratus. There were, however, originally twelve constellations. Their cuneiform names have been preserved, but in the case of Libra, although the name is known, the meaning has not been as yet discovered. The Balance is consequently a modern figure which fills the place of a lost constellation. Hercules is another comparatively modern name. The constellation is called the "Kneeler" by Aratus, and is described thus:—

> Near this, and like a toiling man, revolves
> A form. Of it no one can clearly speak,

> Nor to what toil he is attached, but simply
> *Kneeler* they call him. Labouring on his knees,
> Like one who sinks he seems; from both his shoulders
> His arms are raised; each stretching on its side
> About a full arm's length. And his right foot
> Is planted on the twisting *Serpent's* head.

It is pointed out that while the Kneeler puts his foot on the head of the Dragon, the adjoining Serpent-holder has a foot on the Scorpion, and the suggestion is put forward that these two figures are really different representations of the same person.

It is an interesting point that α Leonis is denoted in Greek, Hebrew, Arabic, and Assyrian by a name derived from the word for "king." We think the probable explanation is that no other bright star is situated so close to the ecliptic. We believe a similar explanation accounts for the importance given to α Virginis in the tablets, the constellation appearing to be subordinated to its brightest star.

We are afraid, however, that although several interesting remarks are made by Mr. Smith, he will hardly convince astronomers that he has accounted for the origin of the constellations.

G. J. B.

NOTES.

Comet Notes.—Comet *b* 1907 was independently discovered by Mr. John Grigg, of Thames, New Zealand, on April 9, several days before its discovery by Mellish.

Mr. Merfield has deduced a set of elements from the Thames observations on April 9, 10, 11, which are given below, that by Misses Lamson and Frederick being repeated, and the orbit of the bright comet of 1742 being also given, as Dr. Berberich has pointed out the remarkable resemblance of its elements to those of *b* 1907:—

	Merfield.	Lamson & Frederick.	Comet of 1742.
T......	1907 Mar. 26·35 G.M.T.	1907 Mar. 27·56 G.M.T.	1742 Feb. 8·3
ω......	327° 1′	328° 47′	328° 24′
☊ ...	189 29	189 7	185 30
i	94 53	110 12	112 45
log q .	9·9627	9·9657	9·8845

The comet of 1742 was first seen at the Cape 1742 Feb. 5, and independently discovered at Paris on March 2. It had then a tail some 5° long, and was followed till May 6. If the two comets are identical, the loss of light on this occasion is very marked. The comet will probably be too faint for observation in June.

Mr. H. A. Peck gives in *Ast. Journ.* 593 a re-discussion of the orbit of Comet 1819 II.; the following is the most probable ellipse, but a parabola is almost as probable:—

T	1819 June 27·7168 G.M.T.	
ω	13° 25′ 57″	1819·0.
☊	273 42 7	
i	80 44 50	
$\log q$	9·53338	
e	0·99988	

Comet 1905 IV. was still visible at Heidelberg on May 10, being of mag. 14. Dr. Weiss thinks that it may be followed till August, as it only fades very slowly; he even thinks there is a chance of recovering it next spring, when it will again be in opposition, and half as bright as last March; if so, it will break all records in the way of prolonged visibility. It will be at distance 8·6 from the Sun, 7·6 from the Earth. A. C. D. C.

MINOR PLANET NOTES.—The following have been found by Prof. Lowell at Flagstaff, Ariz.:—UM^a and UM^b, 1906 June 20; VE^a and VE^b, 1906 Sept. 22; WT^a, 1906 Dec. 6; YY, YZ, ZA, 1907 April 7; ZB, April 9 (the last named was independently found by Metcalf and Coggia); ZG, April 10; ZH, April 15. The following were found by Dr. Metcalf:—ZC, March 20; ZD, April 17; ZE, April 18; ZF, April 11.

The following new planets have been found by Herr Kopff at Heidelberg:—ZJ, May 9; ZK, ZL, May 11; ZM, ZN, May 12.

Another of the new planets proves to have a very interesting orbit; this is XM, discovered by Kopff on Feb. 10 last. This, like 588 TG, has a period and mean distance almost the same as those of Jupiter, the following elements having been deduced by Dr. Strömgren:—

Epoch	1907 Feb. 10·0 Berlin M.T.	
M	335° 47′ 12″	
ω	183 51 52	1907·0.
☊	341 58 25	
i	18 7 17	
ϕ	2 8 24	
μ	292″·584	
$\log a$	0·722504	

Dr. Strömgren points out that this planet does not seem to conform to the equilateral configuration with Jupiter, its discordance from this position being 19°, probably a larger amount than would be covered by libration; also the inclination is very large. He remarks that periodic orbits in the solar system cannot be considered as rigorous solutions, owing to the neglected influence of other planets. Even in this case the configuration with respect to Jupiter will only change very slowly, and the perturbations by the latter will be of extreme interest, and will probably necessitate new developments.

Planet YK = 411, discovered in 1896; YQ = 450 Brigitta; YP=62 Erato; ZA probably=510 Mabella.

TE has been named Marianna. A. C. D. C.

SHORT PERIOD VARIABLES.—The images on the long-exposure Astrographic plates taken at the Paris Observatory are formed by three images, each taken with 30 minutes exposure, the telescope being slightly displaced between exposures, so that the three component images are at the angles of an equilateral triangle. M. Baillaud (Junior), who is the member of the staff at Paris who takes and examines these photographs, has noticed two cases where the three images are of different magnitudes and where this is not the case for all the stars on the plate, which precludes the supposition that the variation is due to such a cause as cloud. The images of a star at R.A. $16^h\ 54^m\ 11^s$, Dec. $21^\circ\ 42'\ 0''$ N., were respectively of magnitude 14·5, 13·0, and 12·8, from which he concludes that this is a variable star whose brightness increases a magnitude and a half in 30 minutes. Another star whose images similarly suggest a variability is at R.A. $19^h\ 0^m\ 8^s{\cdot}44$, Dec. $24^\circ\ 40'\ 16''$ N.

Messrs. Muller and Kemf, of Potsdam, also announce a peculiar variable. This is B.D. $68^\circ{\cdot}200$, which has a period of 1·95 days, and a range of light-variation of only about one-third of a magnitude.

NEW ASTRONOMICAL SOCIETIES. — Perhaps the word "New" should not be applied to the Astronomical Society of Antwerp, for they have lately sent us their second report, but we do not remember to have seen the first. However, the Society seems to be "going" with a good deal of enthusiasm and a good many members. The plan seems to be to have monthly meetings, when a discourse on some branch — perhaps elementary — of astronomy is given. M. Felix de Roy makes a Report on the observing weather of 1906, in which he seeks to vindicate the climate of Northern Europe. At Antwerp the Sun was observed on 304 days, and there were 102 fine nights, not counting partially fine nights, which would have brought the number up to half the nights in the year. The following sentence in this Report caught our eye:—" On n'oubliera pas d'ailleurs que c'est dans la brumeuse Angleterre à Greenwich, près du Londres enfumé qu'on a pu photographier assez régulièrement de satellite de Neptune et les deux nouveaux satellites de Jupiter, dont le plus faible attent à peine la 16^{me} magnitude." Long may the Antwerp Society live to use its fine weather!

We learn of another new Astronomical Society from the *Adelaide Advertiser* of April 11. It seems that the first meeting was held in the house of Sir Charles Todd, who is the President. It was resolved to grant £5 to help to buy an epidiascope, and a very instructive address was given by Prof. Chapman on the "Possibilities of the Telescope."

ROYAL SOCIETY CONVERSAZIONE.—There were a few astronomical exhibits at Burlington House on May 8, but principally interest centred round mechanical applications of the gyroscope. The Solar Physics Observatory sent some stellar and solar spectrograms, and Dr. Lockyer some photographs of clouds. There was an ingenious instrument called a Helio-chronometer, which is a sort of equatorial sun-dial. Two small screens at the ends of a diameter of a circle, whose plane is set parallel to the equator and which can be rotated about a polar axis, have to be adjusted so that the Sun throws an image of a hole in one on to the central line of the other. This observation would give the hour-angle of the true Sun, but by an ingenious cam device the screens are shifted relatively to the zero of the circle, so that the correction is made for the equation of time, and the angle read off is the hour-angle of the mean Sun. A further mechanical adjustment corrects for longitude, so that Greenwich Mean Time is shown at any place.

THE MINOR PLANETS.—In the 'Annals of the Royal Observatory of Belgium' (tome ix. fasc. iii.), M. P. Stroobant makes a statistical investigation of the known asteroids from Ceres to Taurinensis No. 512. He arrives at the conclusions that the maximum density of the asteroids corresponds to the distance from the Sun 2·75, on either side of this maximum the number of small planets diminishes according to the well-known law of probability, and that the asteroids of fainter magnitude are relatively few in the richer zones.

AN ERRATUM.—In the note in last month's number on the Greenwich rainfall (p. 221), the fourth line from end should read "the *minimum* temperature was 53°."

THE post of Chief Assistant at the Cape of Good Hope Observatory, left vacant by the promotion of Mr. Hough to the Directorship, has been filled by the appointment of Dr. Jacob Halm, who has been Assistant at the Royal Observatory, Edinburgh, since 1895. Dr. Halm was Assistant at the Strassburg Observatory from 1889–95, where he took a large share in the observations for and preparation of the Astronomische Gesellschaft Catalogue Zone −2° to −6°. At Edinburgh the meridian-work has been largely in his charge, and the re-reduction of Henderson's Catalogue lately issued from the Edinburgh Observatory bears his name on the titlepage as author. Dr. Halm has also worked at spectroscopy and written on that subject.

WE have to welcome a new contemporary, *The American Astronomer*, of which the first two numbers have kindly been sent us. This is more like a newspaper than most astronomical publications, and, judging from these first two numbers, it does not aim at original articles, but rather at publishing extracts.

THE Visitation of the Royal Observatory, Greenwich, will take place this year on June 8. It has been postponed from the usual first Saturday in June because of the absence from England of several Members of the Board, who attended the Meeting of the International Academy of Sciences at Vienna.

WE regret to have to say that our valued contributor Mr. Denning is in a very bad state of health, and is at present unable to continue his series of articles on the planets. For a similar reason the second part of Prof. Herschel's article on a meteor's path is again postponed.

MR. PERCIVAL LOWELL has organized an astronomical expedition to the Andes for the purpose of observing Mars at the coming opposition. Prof. and Mrs. David Todd and Mr. Slipher form the *personnel*, with Mr. A. G. Ilse, of the firm of Messrs. Alvan Clark & Son, as chief mechanician.

THE details of the will of Mr. C. T. Yerkes, who died at the end of last year, have lately been published. He has left £20,000 to the University of Chicago for the upkeep of the Observatory at Lake Geneva, with the condition that the Observatory shall always bear his name.

MR. EDWARD ROBERTS has retired from the position of Chief Assistant of the Nautical Almanac Office, and has been succeeded by Mr. P. L. H. Davis, F.R.A.S.

DR. ALEXANDER BUCHAN, F.R.S., the meteorologist, died on Monday, May 13, aged 78.

THE next Meeting of the Royal Astronomical Society will be on Friday, June 14; of the British Astronomical Association on Wednesday, June 26.

FROM AN OXFORD NOTE-BOOK.

FOR effective work, for the interest of communications received, for absence of friction in discussion, for number and importance of the congredients, and (last, though not by any means least) for hospitality and even magnificence of entertainment, the third meeting of the International Union for Co-operation in Solar Research, the sittings of which are just concluded at the moment of writing, takes very high rank. The work was effective because it had been prepared beforehand with great care and skill, and vague general discussions were entirely avoided. The communications received included the announcement that the work of fixing a primary standard of wave-length, confided only 18 months ago to MM. Fabry et Pérot, had been completed. The results were in beautiful accordance, and the definitive figures ready for adoption. It was an additional satisfaction to learn that both Prof. Ames and M. Hamy had obtained essentially the same figures by quite independent methods. We astronomers who have become accustomed to variety in result (such as that afforded, for instance, by the investigations on aberration) are tempted to envy the physicists this unanimity. But the temptation passes: for we begin to reflect that they must now try for the next decimal place; and if the accordance persists, for the next beyond that; and so on until differences reappear, and then they must investigate the differences. There is no permanent resting-place in physical measurements any more than in astronomical, but the physicists have found a very pretty and shady oasis in which to halt for a few years.

As regards the numbers present, they were more than double those at Oxford—about 80, say,—many distinguished names being included; and that no friction arose in discussion among so many was due partly to the careful organization and partly to the evident good sense and hearty cordiality of the whole gathering. It is scarcely fitting to describe in detail the hospitable entertainments arranged by M., Mme., and Mlle. Janssen, but it would be ungracious not to say one word of the banquet to 80 people on Tuesday night (May 21), with music and drama to follow—one of the most beautifully arranged entertainments within the experience of such gatherings,—and of the successful day (May 24) at Chantilly, which we shall all long remember. M. Janssen is feeling the weight of advancing years, and is now to retire from active work, though he retains the title of Honorary Director of the Observatory, the executive Director being henceforward M. Deslandres. He leaves the stage with a splendid farewell, wherein he has magnificently inverted the usual relations of beneficiaire to audience. It was eminently appropriate that on the menu-cards of the banquet there should be a representation of the medal struck by the French Academy in honour of the Janssen-Lockyer discovery of 1868. This medal bears the inscription: "Analyse des Protubérances Solaires, 18 Août, 1868," and the reproduction is accompanied by the words "Congrès International des Études Solaires; Observatoire de Meudon, 20 Mai, 1907." A good deal of astronomical history is comprised between those two dates.

M. Darboux, Perpetual Secretary of the French Academy, made a graceful speech at the banquet, and struck a happy note when he spoke with earnest approval of the freedom left by the Union to individual workers, which it has made an article of its Constitution. We knew that we had been striving in this direction, following Hale's lead, but we did not know that others had remarked the endeavour, and it was most gratifying to be patted on the back for it in the elegancies of the French language—to be called, for instance, a model for other Conferences. Naturally we have to disavow the flattering compliment; but it is very pleasant to hear it all the same, and it is astonishing how easy it is to understand French when people say nice things like that.

The minutes and records of the meetings will ultimately appear in irreproachable form, which is only another way of saying that M. Trépied was one of the three Secretaries chosen to take charge of them. Meanwhile it is not easy to give more than a few jottings from memory, since there were no printed agenda or acta of any kind. The Committees already appointed * all made brief and satisfactory reports, and two new Committees were organized:—

(*a*) On solar eclipse work, with Sir Norman Lockyer as Chairman and Count de la Baume Pluvinel as Secretary. This Committee met after the Congrès and decided to make enquiries as to the possibility of observing the eclipse of next January. Intending observers should communicate with the Secretary.

(*b*) On the solar rotation, with Prof. Dunér as Chairman. For the moment I fear I have forgotten who was to be Secretary.

A proposition for another Committee on solar prominence work visually, intended specially to organize the work of observers with small instruments, was unfortunately thrown out. The Congrès learnt with regret that for reasons of health Prof. Young and Prof. Ångström had been compelled to resign the Chairmanship of the Committees on the spectra of sun-spots and on solar radiation. The successors nominated were Mr. Newall and M. Violle. To what was said above about the primary standard of wave-length it should be added that the work on secondary standards is so far advanced that it was decided to authorize the Committee to issue a definitive table without waiting for the next meeting of the Union.

And that brings us to what was perhaps the most interesting resolution of all, the decision to meet in California in three years' time if the Executive Committee (Schuster and Hale re-elected) finds that a sufficient number of European astronomers can undertake the journey. Prof. Hale gave the invitation with the utmost cordiality, but on this express condition, for he frankly said that to continue the success which had attended the meetings hitherto was even nearer his heart than to see a gathering at

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* For a list of these see the 'Transactions of the International Union for Co-operation in Solar Research,' vol. i. p. 175. The Union has no funds for publication, and depends entirely on sales. It is hoped that those interested will buy the volume, which is published by Messrs. Sherratt & Hughes, 34 Cross Street, Manchester. Price 7*s*. 6*d*. net.

Mount Wilson. But encouraged by several definite promises to come, and in the hope of others becoming possible nearer the time, and, finally, with the knowledge that there were many American astronomers for whom Mount Wilson might be more accessible than Europe, he decided to lay the proposition before the Union, and it was accepted in the same spirit. So that in September or October 1910 there will be a fine opportunity for seeing the great observatories of the United States, which I can from personal experience most earnestly recommend others to seize if it is in any way possible for them to do so.

Besides these formal decisions, and others which I may have forgotten, there were of course many interesting communications made both formally and informally. The latter were, perhaps, as important as the former, and far more numerous. A large percentage of the members were carrying parcels of diagrams or bundles of papers under their arms, or were ready to produce negatives or prints from their breast-pockets, showing results of some new line of work or improvements in method. Such accompaniments seemed to be almost necessary articles of apparel or adornment, and those with none to show had an uncomfortable feeling of being in dishabille, especially alongside people like Prof. Hale, who had, so to speak, a new ornament every day. If one may make a complaint about so successful a gathering, it would be that there was not nearly sufficient time to lure these bearers of treasures into corners and appreciate their jewels at leisure. For it is no doubt one of the most valuable elements in such gatherings that a worker gets the chance of enjoying appreciation of a kind which the ordinary method of publication cannot afford: he can put his results before the two or three men whose opinion he regards above those of all others, and he can watch their faces and listen to their comments. If he is so fortunate as to find their approval, what matters in comparison any rebuff from an official superior or from the world at large?

But I fear I am not reporting the details very well. There is a grain of comfort in the thought that I may at least make fewer mistakes than the gentleman who sought us out in the hotel yesterday, and was generously passed on by each man to his neighbour as soon as politeness would permit. After the manner of reporters, he was persistent in leading the conversation away from what we had been doing to the things which interested him, and thus he drew from me (under solemn promise that they should not be put in the Solar Conference report) some remarks on the limitations of space and on radium. Others found themselves talking about motions of stars in the line of sight, and one of them only awoke to the possible results when his tormentor wound up the conversation by genially remarking, "So that was the way Neptune was discovered?"

We had the pleasure of being received by M. & Mme. Becquerel, by the Members of the Institut, by M. & Mme. Lœwy at the Observatory, where there many new devices to be seen; and at the Sorbonne, where there was a lecture by Prof. Julius and a host of experiments and exhibits arranged for inspection, the beautiful colour-photographs of M. Lippmann naturally exciting much interest. Some of us were also fortunate enough to see the private laboratory of one of the few French amateur astronomers, Comte de la Baume Pluvinel, who is working photographically at several photometric problems. He has paid attention to a preliminary of some importance which is usually little regarded, viz. the effect of the interval between exposure and development on the density of the image. The results are quite definite, and important when the interval is small; and it may be hoped that they will be published forthwith. There was also a very complete set of measurements of the intensity of the coronal light, nearly ready for publication. And, finally, Comte de la Baume Pluvinel has initiated a most interesting research on the light of the Moon, arguing that if the Sun is a variable star, its light reflected from the Moon will vary sympathetically and is much easier to measure.

We heard also from M. Baillaud of the enterprise of the University of Toulouse, which is building a mountain observatory on the Pic du Midi. There has been a meteorological observatory there for 25 years, occupied by four devoted observers, who are secluded all the winter and take it in turns to do cook and housemaid work. But an astronomical observatory is now being added and will be completed this year. There is to be a reflector of 25 cm. aperture and 6 m. focus, the mounting being English and the tube having an "optical bench" attached to it at one side, so that the light of celestial objects can easily be treated in a variety of ways. The observatory is at a height of over 9000 feet—half as high again as Mount Wilson—and there are excellent accounts of the climate. But perhaps the most interesting point of all is that there is to be at present no permanent staff, but astronomers of any country are cordially invited to come and make use of the instrument. If I understand M. Baillaud rightly, both bed and board will be provided free during the visit, as well as telescope; but the formal invitation will be issued very shortly by the University of Toulouse. It is to be hoped that there will be a satisfactory response.

After kindly providing a cool period for the meetings of the Union, the weather has suddenly become tropical. Perhaps this will be accepted as an excuse for the many shortcomings of these notes on a most interesting week. I fear the journey to Vienna, where the International Association of Academies is to meet on May 28, will not be typical of the temperate zone,

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. JULY, 1907. No. 385.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1907 June 14.

Mr. H. F. NEWALL, M.A., F.R.S., *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder. Ninety-three presents have been received since the last Meeting, which comprise, amongst other valuable works, Volume II. of the Oxford Astrographic Catalogue, presented by the University Observatory; Potsdam Observatory 'Photographisch Himmelskarte,' Band IV.; Dr. Backlund's Reports on the Russian Mission for the Measurement of an Arc of Meridian in Spitzbergen, presented by the Poulkova Observatory; 20 enlarged reproductions of plates of the Astrographic Chart, presented by the Royal Observatory, Greenwich; six enlargements on glass of photographs of portions of the solar surface taken at the Royal Observatory, Greenwich, and photographs of the solar eclipse of 1905 August, taken at Sfax, Tunis, presented by the Astronomer Royal; nine photographs of Mount Wilson Observatory, and photographs of the spectra of sun-spots, presented by Prof. G. E. Hale.

The President. We have with us this evening Prof. Hale, Director of the Solar Observatory on Mount Wilson, California, and my first impulse was to ask him at once to address the Meeting and tell us something of his work; but on suggesting this to him, he told me that he would prefer that the papers presented, of which there are several of some importance, should first be read, and that he would then occupy the time at his disposal at the end of the Meeting. Under these circumstances, I hope that speakers will limit themselves as much as convenient, so as to give an adequate opportunity of hearing Prof. Hale.

There was a paper by Prof. Turner held over from the last Meeting which I will now ask him to read.

Prof. H. H. Turner. Fellows may remember that I read a paper a couple of months ago on a possible explanation of the phenomena of long-period variable stars showing curves of the light variation, and that the maximum of these curves fell late or early, the explanation suggested being that these were suns like our own, seen in different aspects.

Some of them had equators presented to us, and others had the pole presented, and the solar phenomena with which we are familiar would therefore appear in different sequences owing to the aspect in which the star is presented. I went on then to show, classifying the stars in this way, that the polar stars were absent from near the poles of the Milky Way, and the inference was that the axes of these stars lay mostly parallel to the Milky Way, so that if we look towards the pole of the Galaxy we do not see any ends of stellar axes. After the lecture Mr. Inwards suggested that something similar might happen in the case of double-star orbits, and I asked the help of Mr. Lewis, which was cordially given, as you would naturally expect, and the paper is a joint paper between us; and although Mr. Lewis told me at once that the matter had been gone into a good many times without finding any satisfactory result, the first search seemed to show that we might perhaps hope for more interesting and definite results than had been supposed. The first thing was to form a list of 59 well-determined orbits, which is entirely due to Mr. Lewis. On examining them it did seem that there were more poles of star-orbits (if I may use the term pole to signify a direction perpendicular to the plane of the orbit), there were more poles near the Milky May than far from it. Taking the line of equal partition of the hemisphere, that is galactic latitude 30°, the poles were not equally distributed on opposite sides to it, but there was a slight excess near the Milky Way. The first piece of evidence is similar to that shown for the variable stars, namely, the orbits are arranged in series according to the value of the quantity i or γ, the inclination to the plane of projection, which represents the aspect of the orbit, and is large when the orbit is seen edgeways or equatorially. The galactic polar distances are, as in the case of the variable stars, all large when the pole is presented, but may be large or small when the equator is presented. But we can go a little further in the case of double-star orbits than we can for variables; we can actually assign the places of the poles, the only ambiguity being that each orbit provides us with two alternative poles, a real one and a spurious one. It is well known that we cannot decide between two inclinations of double-star orbits; we have to accept them both as equally probable. A section of the paper is devoted to the question of making due allowance for the spurious poles. Now first of all we find there are, of the 118 poles for the 59 orbits, 67 in the Galactic half and 51 in the other, so that there is a decided excess of poles near the Milky Way. We can make allowance for spurious poles on the hypothesis given in the paper, which shows that they must be distributed nearly equally in the two halves, and this brings out the excess rather more strikingly, because it is an excess of 16 in 59 orbits instead of 16 in 118. Now the rest of the paper shows what happens when we make a rather better selection of material. I don't know how far Mr. Lewis is quite happy about the selection of material, but we found, if we exclude in the first instance all the very small orbits, all those whose major axis is less than a second of arc, that we sensibly improve the excess that we are looking for. If we subtract for the spurious poles an even distribution, we get left 38½ to 20½. Remark that the quite small orbits give an *equal* distribution of 5½ in each section. You see they do not go *against* the theory; they are *indifferent*, and this is what we might expect if the determinations of inclinations are not very good. If now we go further and exclude all orbits with high eccentricity, the remaining 26 orbits of really good material are scattered in the proportion of 21 near the Milky Way as against 5 near the Poles. As regards high eccentricities, it is not unnatural that when the orbit is very elliptic, so that the stars approach at periastron very close together, the difficulties of determining the lines of nodes and the inclination are increased, or there may even be physical reasons for the distribution of inclinations being different. For instance, the planetary orbits in the solar system are all nearly arranged on one plane, which falls in with the law that the axis of the plane lies near the Milky Way, but comets with orbits of high eccentricity are scattered more at random; there may be a physical reason of that kind.

The whole gist of the matter is that an examination of double-star orbits gives results which are consistent with the views advanced on variable stars, and it suggests that we might examine other objects, such as spiral nebulæ, which suggest an appearance of perspective, and there we might find a very large amount of material.

The President. I think we have all followed Prof. Turner's explanation of his paper with great interest, and no doubt we shall be better prepared for its discussion on a future occasion.

The President called upon the Astronomer Royal to make some remarks on two papers which stood in his name.

The Astronomer Royal. These are not yet exactly in the form of papers, but rather in the nature of notes. One of them, written jointly by my son, Harold Christie, and myself, deals with the subject of the illumination of the field of view of a telescope and its effect on the observation of transits of stars.

It is known that with the reversible transit-instrument observations made in the two positions of the instrument generally show discordances. These have been attributed to what is called lateral flexure, and corrections have sometimes been applied for this, but at Greenwich we never felt satisfied that there was any real ground for this assumption. We have suspected sometimes that it was due to some looseness in the object-glass, which in one longitude determination was actually found to be the case, but though in succeeding longitude operations precautions were taken to prevent such an accident, this puzzling discordance remained. The striking point about it is that it varied from night to night and at different times. Whilst there is a general tendency in one direction, there are considerable variations in the difference of the clock-error as determined in the two positions of the instrument, as I will show presently on the screen. My son was working with the small reversible transits to get them into working order with a view to the determination of the longitude of Cairo, and found these differences. Mr. Hollis, when previously thinking about this matter, had come to the conclusion that to obtain consistent results it was necessary to keep the focus of the eyepiece absolutely fixed, and not only the focus, but since the eyepiece used is of the diagonal or *coudé* form, that it was expedient to keep the eye-tube, which is liable to sag, rigidly attached to the tube of the telescope. It may be difficult, however, for some persons to keep the eye-lens always in the same position for focus, owing to change of focus in the eye. My son made experiments by setting the eyepiece at a position which he called "good" focus, and then at a position outside the focus, but not sufficiently to make observing impossible, and thirdly in the similar position inside "good" focus. Series of observations of stars were made with the eyepiece at these three settings, and in the extreme settings the differences of clock-error in reversed positions of the instrument were of opposite sign, and were also much larger than were the similar differences in the mean position. Thinking this over, he came to the conclusion, as I did independently, that this might be due to inequality in the two sides of the reflector which illuminates the field of view, and by which the wires are seen, and as a result of that I have arranged a different form of illumination which I hope has quite got rid of the discordance. With the older form of illumination, which consists of an annular reflector in the axis of the instrument, my son found that by varying the illumination he could see the wires move relatively to the star, and he also found the same thing happened by changing the focus, which I have verified by observation of a mark. I will explain how it has struck us by this diagram, which is supposed to represent the annulus. Of course it is realized that we do not see the wires themselves by rays emanating from them, but what we see is the shadow of the wire as cast by the illumination of the field, and it is all-important that the illumination should be good in order that we may get sharp definition of the wires, not merely bright enough, but that we should have a good pencil of rays. [The Astronomer Royal went on to show, by means of the diagram, how the shadow of the wires is formed, and that when the eyepiece is either inside or outside the focus a double image is seen.] That is a thing that has puzzled me a long time; it is very distinct with this form of illumination, and I think the general idea was that it was peculiarly inherent in the Ramsden eyepiece. Now on testing this with our transit-circle, I found, as I expected, that there was a very distinct double image, but when, instead of illumination by the annulus, we used daylight through the whole of the object-glass, we did not get a double image but a sort of multiple image, or there was a tendency on one side to two images, but it was filled up between, rather stronger on the outside, and that seemed to point to the annulus as the cause. Then in the case of inequality in the illuminating power of the reflector, that would, of course, make one of these images stronger than the other, and as a matter of fact, in this reflector, which has gold-leaf stuck on to it, one side is markedly more reflective than the other, so that presumably the one image is seen and the other is lost sight of. At any rate, there is the fact that as the eyepiece is drawn out or pushed in, the image of the wire changes its position relatively to the star or other object. There is another form of illumination, which consists of a small prism cemented on to the object-glass; but there are drawbacks to that, so I have adopted a plan which is a modification of this form of central illumination, with some new points about it. In the first place, instead of having a reflector, there is a matt surface which scatters the light in different directions quite uniformly; there is a condensing-lens which throws the rays from a small electric lamp on to this matt surface, and then it comes to the centre of the field. The aperture of the illuminating pencil is as large as practicable (1½ inches or more for an 8-inch object-glass), so as to diminish diffraction effects and give sharp definition of the wire. The difficulty was the method of attachment to the object-glass, and for that purpose there is a piece of plate glass worked to the same curvature as the object-glass and cemented to it, which is used as the base of a hollow cylinder, of which the opal glass reflector is an oblique section.

The Astronomer Royal proceeded to show tables of differences, clock slow west – east, from observations made with the two methods of illumination, the collimation determined by nadir observation being used in the reduction. The differences of the observations made with the annular illumination were in a few cases as large as three-tenths of a second of time, and showed some variation, but with the central illumination the differences were all less than $0^{s}\cdot04$. It was pointed out that the measured inequality of pivots was sufficient to cause differences of this size.

Sir David Gill. I am much interested in the paper because the plan of illumination which has been arrived at by Sir William

Christie is precisely what I arranged at the Cape 28 years ago *.

Mr. H. P. Hollis. It is a long time, Sir, since this subject was mentioned in this room, but once it was a not infrequent subject of discussion, and the problem of the line of collimation of a reversible transit-instrument is an old one, dating back at least to Wilhelm Struve, who discussed it in connection with the meridian instrument of the Dorpat Observatory. If I may be allowed to state the problem it is just this:—The error of collimation of a transit found in the ordinary way described in the text-books, that is by observing a distant mark in the horizon and reversing, or, as in the case now before us, by treating the nadir as the mark, does not satisfy the star observations; or, in other words, the star observations give a different error of collimation to the mark or nadir. Struve suggested that this was due to flexure or bending of the axis, and this is generally put forward as the cause when discordances of this kind are found, but it is not always easy to prove. When the subject was last mentioned here, which was in the year 1884, the late Mr. Stone said, as other people have said, that the way to treat a reversible transit was to treat it as two instruments, that is to say, a different collimation and different errors should be used in the two positions of the instrument; but if you do that you give up the essential principle of the instrument which makes it so effective, for the reversible transit is in its essential principle the most valuable of all instruments for the determination of absolute time. Following that discussion, Sir George Airy wrote a note to say that after much consideration he had come to the conclusion that these discordances were due to inequalities of the pivots and bearings—that a workman could not work with the exactness required in such a delicate astronomical instrument. So that there are two main causes put forward to account for these differences, flexure and pivots, and now the Astronomer Royal suggests that the cause is an optical one. I know that for many years past the Astronomer Royal, when instructing observers, has realized the importance of accurate adjustment of the lighting, but somehow, with the best of intentions and the greatest care, we have not managed to attain complete success, so that we are compelled to think that it must be the fault of the instrument, and this appears to have been remedied by the new way of lighting the field. I should like to point out that the large differences in the table relating to the annular illumination do not affect the correctness of the result if everything is kept symmetrical all the time, as it should be †, but it is evidently more reassuring if these differences are small, as you see they were after the object-glass illumination was installed.

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* [The method of illuminating the field of view of a telescope by a reflector at the object-glass was adopted by Airy for the Water-telescope set up at Greenwich in 1870. It does not seem that this is claimed as a new principle in the paper.—Eds.]

† [In explanation it may be added that on this hypothesis of a lateral movement of the apparent wire, the line of collimation deduced from reversal

I do not mean to imply that this is the first time that an optical reason has been given as the solution of the problem, for we remember the trouble that the officers of the Indian Survey had with this question of collimation, and how Colonel Burrard traced this trouble to an optical source, but the problem was then not quite the same as this, nor was the solution; nevertheless, it seems that the optics of so simple an instrument as a transit is well worth consideration.

Prof. Turner. As I understand this method, it involves attaching an electric lamp near the object-glass of the instrument. The puzzle to me is how this was done when the method was adopted by Sir David Gill 28 years ago. Did he have the electric light then?

Sir David Gill, in answer to questions by the Astronomer Royal, explained the method of illumination which he had arranged at the Cape.

The President. May I return thanks to the Astronomer Royal for what he has brought before us, and express to him the pleasure it gives us to welcome a new recruit to the ranks of those who are helping forward astronomical observation?

The Astronomer Royal exhibited two diagrams on the screen showing the positions of Jupiter's Sixth and Seventh Satellites resulting from measures of photographs taken with the 30-inch reflector at Greenwich during the Oppositions of 1905–6 and 1906–7. He explained the difference between the apparent orbits of the Seventh Satellite at the two Oppositions owing to the change in the position of Jupiter relative to the Earth. In the first case the Seventh Satellite moved in the opposite direction to the Sixth, but at the next Opposition the motion was in the same direction.

Rev. W. Sidgreaves. All spectroscopic observers of Mira Ceti have noticed the great change in the green-blue hydrogen line Hβ at the recent exceptionally bright maximum of last December. This is not the only change in the apparent character of the star's hydrogen radiation: Hδ in 1897, and again in 1898, was quite as intense as Hγ, without allowing for the sensibility-curve of the photographic film; in 1906, as seen on the screen, it is distinctly the weaker of the two.

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observations on a mark, on the nadir, on polar stars, or on time stars *is the same line in the telescope in all the cases* if the position of the eyepiece is the same before and after reversal, *even though the reading of the micrometer is different owing to this line of sight being different in the different methods.* This being so, the level error, the azimuth error, and the clock error found by taking means of results in the two positions refer to the same line throughout, if it be assumed that there is no malformation or inequality of pivots or bearings. —H. P. H.]

The present paper deals almost exclusively with these bright lines. The absorption line-spectrum appears on our plates substantially the same as on those of the previous years mentioned; but there are considerable changes in the band-spectrum, which will be mentioned presently in connection with the hydrogen lines.

In 1897, and again in the following maximum of 1898, the peculiarity of the hydrogen-spectrum appeared in the sequence of intensities being the reverse of that shown in the laboratory. In the laboratory the crimson line Hα is much the strongest on the photographic plate, and the rest fall off in alphabetical order continuously. In Mira this order was reversed in the only three lines we were then able to photograph: Hδ was the strongest, Hγ next, and Hβ was extremely weak. In 1906 Hγ is apparently the strongest by the same prism and on the same quality of plate (Edwards isochromatic); but the real order of decreasing radiant energy on the plate is shown by another prism, not in service on the former occasions, to be the same, with less contrast, as in the earlier photographs. The spectrum now on the screen is by a small objective prism which does not absorb the shorter wave-lengths lost in the compound prism; and by it Hδ is quite equal to Hγ, without allowing for the less sensibility to the Hδ radiation.

By this prism we have other noteworthy peculiarities of the hydrogen spectrum, but without the means of comparing with former years. In the strongest of our recent photographs the hydrogen lines are seen up to Hρ, excepting, as usual, Hε, of which there is no trace even on this strong photograph, while Hζ is a very prominent line. The three next lines, Hη, Hθ, Hι, are out of all proportion weaker than Hζ. Then follow the remaining six lines, differing little from one another in intensity, and again out of proportion weaker than Hι. They are indeed extremely weak, but they were easily pointed with a low-power micrometer eyepiece, and all are bright lines.

We are not yet in the position of being able to answer the all-important question whether these peculiarities of the hydrogen spectrum are to be attributed to the condition of the star's hydrogen envelope or to absorbing effects of an exterior atmosphere. The absence of Hε has been admitted to be the effect of the sympathetic absorption of calcium vapour arresting the Hε radiation, and this explanation is confirmed by the objective prism in 1906, which shows the two strong absorptions at H and K of the solar spectrum, in addition to the blue line at λ 4227.

Can we, now, set down the other irregularities and changes to effects of absorption? There is some evidence in the affirmative. In 1897 Hβ was covered by an absorption-band (numbered 14 in our wave-length table for that year*), which appeared to be an extension of Dunér's band 9. In this band there was a feeble division near the position of Hβ, and noted at the time as "presumably bright Hβ." In the photographs of 1906 all the absorption-bands are much weaker, as seen clearly on the screen. And this band no. 14, together with the two more refractive bands numbered 13 and 12, has disappeared; all three have lost their shading extensions, and appear as strong stout lines in the positions of the former sharp edges, and Hβ is set free from the absorption of band 14.

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* Monthly Notices, lviii. p. 350.

But one cannot rest content with this as an adequate explanation. Hβ ought to be too strong to be so completely stopped by the weaker part of the absorption-band of 1897, and, on the other hand, absorption fails to account for the changed condition of Hδ with respect to Hγ. There are no absorption-bands in these regions, and the regions themselves show no change; they are the same in 1906 as in 1897.

It seems therefore at least probable that the changes in Mira's hydrogen spectrum are really due to changed conditions of its hydrogen surroundings; and this should be kept in mind whilst considering a further question of still greater moment, viz. whether the extraordinary formation of the hydrogen lines, brought out by the objective prism at the recent bright maximum, are of stellar or photographic origin. As seen on the screen, Hδ has a decided wing on its red side, shading down, but terminating in a bright line. The same formation is seen in Hγ with less separation by smaller dispersion, and also in Hζ, but rather too weakly to be thrown upon the screen.

Against the stellar origin of this formation stand the facts: (1) that there is no clear sign of the same character on the plates exposed in the compound-prism spectrograph; (2) that the formation is exaggerated both by over-exposure and by inferior definition; (3) that of two plates exposed in succession in the prismatic camera, one with excellent definition by a quick trail shows no wings to the lines, the other with inferior definition by a very slow trail shows the formation very strongly. These are the two plates which for a time decided the question for me; they seemed to prove the already suspected instrumental treachery. But, paradoxical as it may seem, it was by the same two plates that I was led eventually to the conclusion that the formation of the lines was really of stellar origin. In the weaker photograph I detected an unmistakable, though feeble, bright line on the red side of Hδ, which on the micrometer stage was found to be precisely in the position of the bright line termination of the wing in all the stronger photographs; and, further, the measured intervals between the strong centres of Hγ, Hδ, Hζ, and the bright line terminations agreed in showing the same spectral interval of 5 tenth-metres. These two findings saved the series of 12 plates, at least in my judgment, from the reproach of a treacherous conspiracy, and I no longer hesitated to submit the facts to the Royal Astronomical Society. The absence of any confirmation by the direct compound prism may be accounted for by quicker trailing and less dispersion.

A prolonged exposure with one of the more powerful instruments might have shown Campbell's triple formation greatly magnified, for there is no inherent improbability of spectral manifestations, equal to those of a new star, in one which rises from the 9th to the 2nd magnitude.

The President. Of all the valuable information that Father Sidgreaves has given us, the fact that interested me most was the absorption of the bright hydrogen line by calcium vapour. Much as I am longing to hear Prof. Hale, I think I must first ask Prof. Fowler to tell us something of his paper, which may have some bearing on Prof. Hale's work.

Prof. A. Fowler gave an account of his identification of the flutings of magnesium hydride in the spectra of sun-spots. The brightest of these flutings begins in the green at 5211·0, and the structure-lines composing it extend nearly as far as the F line. Measurements of the component lines show a close correspondence with "band-lines" or "umbra-lines" which appear in the spots. The head of the fluting at 5211 in the spots was first recognized as such by Mr. Newall, and there is another head near the more refrangible edge of b_1, which is also the beginning of another fluting clearly shown in the Mount Wilson photographs of spot-spectra. Among many others, the well-known double umbra-lines near 5163, 5160, and 5157 are well represented by components of the magnesium hydride flutings. Additional photographs are required before it can be ascertained exactly how much of the detail of the spot-spectrum is due to magnesium hydride, but there are indications that some hundreds of the umbra-lines will be accounted for. The identification is of interest as supporting the view that the absorbing vapours in spots are at a relatively low temperature, inasmuch as chemical compounds may exist without complete dissociation (Prof. Hale has already shown the existence of titanium oxide in spots). As a great number of the umbra-lines under consideration have been found by Prof. Hale to correspond with faint lines tabulated by Rowland, it must be inferred that magnesium hydride not only occurs in spots, but in a less degree in the general reversing layer of the Sun.

The President. Prof. Fowler has made the plot thicken by this two-fold identification. I will not call for discussion, but will at once ask Prof. Hale to speak to us, and will give you the opportunity of according to him a very warm welcome.

Prof. Hale (who was received with applause) gave an address on the Mount Wilson Observatory, California, and the work done there. He showed pictures illustrating the inaccessibility of the situation, and one of the specially designed motor-waggons for carrying instruments and building-materials to the Observatory. He described the arrangement of the parts of the Snow horizontal reflecting telescope with mirror 30 inches in diameter, and explained the method adopted (in a vertical coelostat telescope now under construction) to prevent change of the figure of mirrors by impinging of the Sun's rays. The apparatus consisted of a series of reflectors to make the Sun fall on the back of the mirrors as well as on the front. Prof. Hale exhibited photographs of the 60-inch reflecting telescope, now nearly completed, and spectrograms of the solar surface and sun-spots, dwelling on the fact that the Mount Wilson photographic studies of spot-spectra give results which are in good general agreement with the results obtained visually by Prof. Fowler at South Kensington. He also called attention to the difference in the spectrum of the solar surface at the centre and near the edge of the disc. Comparison of spectroheliograph pictures of the Sun taken in calcium and hydrogen light led to conclusions as to the height of the flocculi, and it was suggested that the area of the flocculi should be systematically measured.

The President. There is surely no one who contributes more effectively than Prof. Hale to the study of the subject for the promotion of which our Society is established. He has given evidence of that in what he has shown us to-night. If there is anyone who promotes the study more actively we hope to see him often and soon. Meanwhile, I should like at this moment to ask you to return your thanks to Prof. Hale. The minutes have slipped by, but as there was no motion in the room I did not wish to interfere with the remarks made by Prof. Hale. I do not know whether there is anyone who would like to ask questions.

Prof. Turner. There are many things we should like to ask Prof. Hale, but while we can all bear with him, we might not be able to bear with ourselves. Therefore I trust it will not be misunderstood if at this late hour we do not rise to speak.

The President. I will proceed then to return our thanks to Prof. Hale. I myself mean to write to him on several points.

The following papers were announced and partly read:—

W. S. Franks. "Note on the Colours of α and ο Ceti."

W. S. Franks. "The Relation between Star Colours and Spectra."

E. E. Barnard. "On the 'Owl Nebula,' M. 97, N. G. C. 3587."

P. H. Cowell. "Ancient Eclipses."

Rev. A. L. Cortie. "Note on the Visual Spectrum of Mira Ceti in December 1906."

Rev. W. Sidgreaves. "The Spectrum of Mira Ceti in December 1906, as photographed at Stonyhurst College Observatory."

Royal Observatory, Greenwich. "Observations of Comets *d*, *e*, *g* 1906, from Photographs taken with the 30-inch Reflector of the Thompson Equatorial." (Communicated by the *Astronomer Royal.*)

Rev. T. E. R. Phillips. "Observations of Jupiter, 1906–7."

T. E. Heath. "Description of an Equatorial Reflecting Telescope driven by a Hydraulic Ram."

A. Fowler. "The Origin of certain Bands in the Spectra of Sun-spots."

P. H. Cowell and A. C. D. Crommelin. "The Perturbations of Halley's Comet."

Sir W. Christie and H. A. Christie. "The Illumination of the Field of View, and its Effect on Observations with a Transit-Instrument."

Prof. Anders Donner, Director of the Observatory, Helsingfors, Finland, was balloted for and elected an Associate of the Society.

The following gentlemen were elected Fellows of the Society:—

Major-General Henry Herbert Lee (late R.E.), The Mount, Dinas Powis, near Cardiff, South Wales.

Augustus Edward Hough Love, D.Sc., F.R.S., Sedleian Professor of Natural Philosophy, Oxford.

Rev. Reginald William Bickerton Moore, M.A., Vicar of St. James's, Bath, 11 Devonshire Buildings, Bath.

Herbert Gerard Tomkins, Government of India Finance Department, Lahore, India.

The following Candidates were proposed for election as Fellows of the Society:—

Burdett Mason, Larondouette, Bayonne, France (proposed by W. E. Wilson).

Arthur William Meers, F.R.G.S., Merchant, Lugano, Wickham Park, Beckenham, Kent.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Sion College, Victoria Embankment.

Wednesday, 1907 May 29.

F. W. Levander, *President*, in the Chair.

Secretaries: J. G. Petrie.
H. P. Hollis (*vice* J. A. Hardcastle).

The Minutes of the previous Meeting were read and confirmed.

The list of presents received was read by *Mr. Hollis*, and the thanks of the Association were accorded to the respective donors.

The names of three Candidates for Membership were read and passed for suspension, and the election by the Council of four new Members was unanimously confirmed.

The President then read the following extract from a letter that he had recently received from the Secretary of the New South

Wales Branch of the Association:—"We are forming a Mars Section this winter to make the most of the forthcoming favourable opposition. I expect that good results will accrue, as our instrumental equipment is fairly adequate, being three 6-inch clock-driven refractors, 4¾- and 4½-inch clock-driven refractors, and several 3¾-inch altazimuth refractors. Then we have the following clock-driven reflectors: one 18-inch, one 12-inch, one 10-inch, and four 8½-inch. All these instruments are first-class optically."

Mr. H. P. Hollis read a paper by *Mr. Scriven Bolton* on "Partial Occultation of the Shadow of Jupiter's Second Satellite by the First Satellite, and its apparently Variable Rate of Motion." The paper described an observation on February 21 last, when Mr. Scriven Bolton saw Jupiter's Satellite I. and the shadow of Satellite II. in transit simultaneously. They came on the eastern side of the disk practically at the same moment and in contact. The shadow of the first moved westward faster than the satellite, but later it slowed down, and the satellite gained on the shadow, so that they again overlapped. Mr. Scriven Bolton also noted that the equatorial diameter of the shadow was longer when it entered the disc than at egress. Mr. Hollis added that this paper showed careful observation on the part of Mr. Bolton, but the phenomena were not inexplicable — they could be explained geometrically. By the help of the blackboard an explanation was given of the apparent variations of the length of the shadow as it crossed the disk.

The President said that Mr. Scriven Bolton had not sent any slides, but some very good illustrations accompanied the paper and would be reproduced in the Journal.

A Member enquired whether there was any explanation of the apparently variable speed of the shadow or of the satellite.

Mr. Hollis said no explanation was suggested, but he thought it could be accounted for by reasons very similar to those just given.

Mr. W. T. Lynn then read a paper on "The Surfaces of the Inferior Planets." He said that it was now generally understood that, while Mercury had no atmosphere capable of producing any appreciable optical effects, Venus possessed an atmosphere denser than that which surrounded our Earth, and that appeared so uniformly laden with cloudy strata that nothing could be seen of the true surface, and the rotation-period remained, and was likely to remain, an unsolved problem. Prof. Schiaparelli's conclusion that Mercury rotated in the same period of time in which it revolved round the Sun was, the Author believed, now universally accepted. But a similar conclusion in the case of Venus was contested. The point raised was that if the same side of Venus were always presented towards the Sun, the condition of her atmosphere on that side must be enormously different from that on the other, and as we saw different parts of each at different times, surely some indications of the changed appearances should be perceptible to our scrutiny. This argument, if admitted to be valid, would be in favour of a short rather than a long duration of rotation.

Mr. Gavin J. Burns read a paper on "The possible Absorption of Light in Space." Mr. Burns said that, assuming that light in its progress through space was absorbed, the absorption might be conceived to take place in one of two ways. The luminiferous ether might itself act the part of an absorbing medium, or the rays of light might be obstructed by material particles in the form of cosmical dust. In his opinion, the idea that light might be lost in being transmitted by the ether was not a legitimate scientific hypothesis. There was no independent evidence in its support, and the assumption was more difficult to explain than the phenomena to be explained. With respect to the obstruction of light by material bodies, we had independent knowledge of the existence of such bodies in the form of meteorites, and the distribution of cosmical dust might be regarded as probable. The only question to be considered was whether cosmical dust existed in space in such quantities as to materially reduce the apparent luminosity of the stars. If it be assumed that all stars are themselves of the same size and brightness, since some faint stars are near us judging from their proper motions, on this hypothesis it would follow that only about 4 per cent. of the light emitted by a 9th magnitude star reached us. It seemed obvious from the above considerations that the quantity of dark absorbing matter must be much greater than that of the stars themselves, which appeared on the face of it very unlikely. Upon the whole, it seemed to be very improbable that the light coming to us from the stars was sensibly diminished by absorption, and hence all stars are not of the same intrinsic luminosity.

Mr. E. Walter Maunder said that Mr. Gore, in a recent article on "Some of the Paradoxes of Astronomy," had pointed out that, if they supposed that the nebulæ extended on the average equally in all directions, it followed that the further off they assumed a nebula to be, the greater they must assume its total mass, and not merely so, but the greater they must conclude its gravitational effect upon us. Something like the same argument might possibly be applied to the question of the distribution of meteoric dust throughout space. He thought therefore that they would be led to the conclusion which Mr. Gavin Burns had put before them, namely, that it was impossible to suppose that any very serious part of the light of the stars was lost by absorption by meteoric dust.

Mr. Seabroke remarked that, in estimating the quantity of material required, if equally distributed, to shut out the light of a star, one must consider that the space to be filled up was a cone having for its base the star and its apex the eye. It required a very considerable quantity of dust to sufficiently fill that space, and it would have a considerable mass.

The President said that in the last number of the 'Journal' there was a reproduction of a photograph of the Sun taken by Mr. Neate with a 3-inch Watson-Conrady achromatic. Father Cortie was extremely pleased with the photograph, and had sent three others taken by Mr. Neate with the same instrument on February 3 and 10 and April 1. These were then thrown upon the screen and described by Mr. Maunder.

Mrs. Maunder then gave an account of her paper on "An apparent Influence of the Earth on the Numbers and Areas of Sun-spots in the Cycle 1889–1901," presented recently at a Meeting of the Royal Astronomical Society, an account of which appeared in the June number of the *Observatory*.

The President said he was sure that all the Members would join him in congratulating Mrs. Maunder on the result of her investigation. On behalf of the Association, he would tender hearty thanks to Mrs. Maunder for her kindness in coming there and telling them of the work in which she had been engaged.

At the request of the *President, Prof. C. Michie-Smith* gave an account of the work being done at the Kodaikánal Observatory, South India, of which he is the Director. He also described the instruments used in the work, and his address was illustrated by a series of lantern-slides. In conclusion, he reminded the Meeting that two Members of the Association, Mr. and Mrs. Evershed, had recently joined the Observatory staff. He trusted that in the near future they would be able to add greatly to the knowledge of the physics of the Sun by their united work.

Mr. G. J. Gibbs then described his helio-chronometer, which he claimed to have some advantages over any of the older forms of sundial.

The Meeting adjourned at 7^h 10^m P.M.

ROYAL METEOROLOGICAL SOCIETY.

THE second of the afternoon Meetings for the present Session was held on Wednesday, June 19, at the Society's Rooms, 70 Victoria Street, Westminster, *Dr. H. R. Mill*, President, in the Chair.

Mr. F. Campbell Bayard read a paper on "Weather and Crops, 1891–1906," in which he gave an analysis of the Agricultural and Horticultural Tables which are included in the annual 'Phenological Reports.' He had sorted out the various crops into "good," "average," or "bad" for each district, and against each he had placed the temperature, rain, and sunshine for the four seasons, and whether these statistics were above or below the average. In the paper, Mr. Bayard gave tables showing the general results with regard to wheat, barley, oats, beans, peas, potatoes, turnips, mangolds, hay, clover, apples, pears, plums, raspberries, currants, gooseberries, and strawberries.

A paper by *Dr. C. P. Hooker* on "The Relation of Rainfall to the Depth of Water in a Well" was also read. The Author gave the weekly measurements of the depth of water in a well 101 feet deep at Further Barton, Cirencester, compared with the weekly rainfall for the years 1903–1906. The results were very interesting, as they included the remarkably wet year 1903 and the droughty summer and autumn 1906.

Mr. Walter Child exhibited his "Step" anemometer, an instrument which he had designed to obviate the "sheltering" error of the Robinson's cups.

Alexander Stewart Herschel.

ASTRONOMERS in general will have heard with regret of the death of Prof. Alexander Herschel, a member of the famous family happily not yet extinct, whose traditions are being worthily sustained by its present representatives. Those who are in the habit of attending Meetings of the Royal Astronomical Society and similar gatherings will miss a familiar and striking figure, and we of this Magazine have to regret the loss of a valued contributor, whom we shall always remember for his willingness to help when called upon, for his modesty in volunteering contributions, and for his punctilious courtesy in correspondence and in affairs generally. Though apparently reserved in manner, he appears to have made many friends in passing through life, and for this *in memoriam* article it has seemed well to ask some of these to speak in turn of our late friend in the period during which they knew him. Prof. Lebour's paragraph carries the history to the year 1886. Since that time Prof. Herschel lived in the family house at Slough, occupying himself in observing meteors, in occasional literary work of an astronomical kind, and in attending scientific meetings. The paragraph of dates and events is a copy of the Professor's own handwriting, made by his nephew, Rev. J. C. W. Herschel, to whom we are indebted for this and other help, and who writes about his uncle:—

"He was always a most kindly man, so thoughtful for others, invariably bringing some small token on his visits to give away, especially to children, and averse to receiving any himself. Witness the legacy which someone left him, and which he at once spent anonymously on Durham University."

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HE was clever with his head and skilful with his hands, and as a boy he was fond of Natural History and such things and was clever at stuffing and setting up birds. He was athletic and active and a good swimmer. As was the case with his two brothers, he was sent to the Clapham Grammar School kept by the late Prof. Pritchard, and distinguished himself in mathematics. He then went to Trinity College, Cambridge. While at Cambridge he was always busy and active, but his interests were numerous, and his mathematical reading was of too desultory a nature and too little directed to "paying" subjects to permit of his taking a degree at all corresponding to his capacity and ability: he came out as 20th Wrangler in the Tripos of 1859. His taste for Natural History was exhibited by keeping for a few days loose in his College rooms a dozen Death's Head Moths, to test their habit of uttering a sharp cry when touched. His taste for models and experiments was shown in many ways: at one time he hung a box of dry sand from the ceiling of his room, the sand dropping through a hole in the bottom and tracing curves on the floor, to illustrate problems connected with the rotation of the Earth: this experiment occupied 2 or 3 days, during which time he lived in another man's rooms. He helped Prof. Maxwell very much in his well-known experiment with "The Devil on Two Sticks"—a double cone, which was whirled into a violent condition of rotation by means of a cord and two sticks. Herschel made the model and worked it with great effect in the Great Court. The double cone was taken off the cord, while rotating, by a short loop of string passed round a nail at one end, and the cone then stood out horizontally and turned slowly round a vertical axis in illustration of the Law of Precession. His passion for experiments and models in illustration of the meaning of equations remained with him all his life. After leaving Cambridge he was appointed a Professor at Newcastle.

A very pleasant feature in Herschel's character was his readiness to help others with his knowledge and skill. A large part of his lifetime was taken up in working problems for other people out of sheer good-nature, and frequently problems in which he took no particular interest.

He took up the Meteor observations and speculations at an early date, certainly as early as 1860, for in that year I assisted him by observing Meteors on Ipswich Racecourse, and kept up his observations and speculations concerning them all the rest of his life. But he was a very modest man, and the amount of work which he gave to the Meteors as well as to Science generally is I think, very little appreciated. And in whatever he took up he helped himself most cleverly with models and simple instruments which he made for himself. W. Airy.

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As my contribution to the notice in memory of my old friend Alexander Herschel, it will, I trust, be sufficient to refer to his extraordinary enthusiasm and tireless devotion to work during the whole of his residence at Newcastle, from the foundation of the Durham College of Science in 1871 to 1886, when he finally left for the South.

He was provided with probably the worst Physical Laboratory that any Professor of Physics has ever been asked to work in. This, however, he fitted with apparatus of all kinds, much of it of historical interest, and much of it made with his own hands—stowed away in every conceivable receptacle, for space was altogether inadequate, and here he worked all day and every day, and generally far into the night, preparing experiments for the next day's lectures and demonstrations. He was not by any means averse to society, and frequently went out to dinner, &c., but after such functions were over he usually finished the night alone in his laboratory. Indeed, nothing more strenuous than his work in those years can be imagined. He left the North before the College migrated to the new buildings, which have only recently been finished, but he left the apparatus which he had accumulated from all sources as a gift behind him, and the new Physical Laboratory of Armstrong College is named the "Herschel" after him. I can bear witness to his delight when, on the occasion of the King's visit last July, he examined the details of the Department he had founded. There was not a grain of selfishness or jealousy in Herschel, and he rejoiced in the luxurious housing of his successor, quite forgetful of the discomforts of his own times.

Besides his regular work as Professor of Physics, Prof. Herschel, whilst in the North, kept up a constant and very large correspondence with observers of meteorites all over the world, the results of which are (only partly, however) to be found in the Reports of the British Association Meteorite Committee, of which he was Secretary. He also carried out long series of experiments on fluorescence, on the conductivity for heat of many rocks, on electrical storage, on films for photography, on filaments for incandescent electric lamps, and in many other directions. Most of the above-mentioned investigations have borne fruit, but some, I am afraid, never reached publication. He spent as much time as he could spare in the late Mr. R. S. Newall's Observatory at Gateshead (and at that of the Rev. J. M. Perry at Alnwick, I believe), where the best telescopes and other astronomical appliances were always placed at his disposal by his friends.

He took frequent part in the discussions at the Meetings of the North of England Institute of Mining Engineers, of which he was Honorary Member, and constantly helped in the scientific work of that Association. He was an active Member of several of its Special Committees, and much labour was spent by him in making experiments for them on fans for mine ventilation, in designing apparatus for registering earth-tremors, and on many other subjects of applied science.

Altogether I should say that no man ever worked more incessantly or *gave away* his work more readily to anyone who could, or thought he could, make use of it; and no one did so more generously or with a more utter indifference to thanks. If Herschel had patented one-hundredth of the inventions or improvements he was constantly making or contriving for others, he would have been a rich man indeed. But no man ever cared less to make money than he. He was a thoroughly open-hearted man, kind to a fault, and one whose great worth was largely withheld from public ken by the absolute modesty and under-estimation of himself, which were among his many personal charms. That he was somewhat eccentric in many small matters was obvious on very slight acquaintance, and only those who had the privilege of knowing him long and intimately could realize what great gifts of heart and intellect and what amazing courage were hidden beneath his mere outward characteristics. G. A. Lebour.

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Meteoric astronomy has indeed lost an ardent, able, and experienced student in Prof. Herschel. During a very long series of years he not only made observations himself but collected, compared, and discussed the results of others.

Prof. Herschel was a prolific writer and a correspondent of exceptional kind, his letters being exceedingly long and highly interesting. His desire for the truth often led him to enter minutely into details. In the observation of meteor-flights he attained a high degree of accuracy, and in the calculation of the real paths of Fireballs he probably stood alone, for no one could deal so well with discordant observations and treat them as effectively, often getting very satisfactory results from apparently incongruous data. Though Prof. Herschel did not habitually employ a telescope in astronomical observation, nor systematically sweep the firmament for interesting objects as his father and grandfather had done before him, he yet accomplished very meritorious work with the unaided eye, and meteoric astronomers will ever be deeply indebted to him for the extent and valuable character of his labours. W. F. D.

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[*In a letter to Miss Herschel from a friend of the family.*]

I trust that he died without pain, and I feel sure, from his well regulated career, that his mind accepted the inevitable with submission and peaceful trust. I have been told that he willingly lent his valuable mathematical aid to foreigners, astronomers and others, quite regardless, so long as the lamp of knowledge was handed on, that it should be known by whose hand it was transmitted. This is a rare quality amongst gifted men, and I think no one could meet the Professor without being struck with the fact that one who knew so much should be so simple and unassuming.

John W. Sherer.

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Alexander Stewart Herschel, second son of the late Sir John Herschel, Bart. Born in Cape Colony in 1836, and educated at Clapham Grammar School, 1851–55, and at Trinity Coll., Camb., 1855–59, and at the Royal School of Mines, London [1861–? 65]. B.A.Camb., 1859; M.A.Camb. and (*ad eundem*) Durham, 1877; Hon.D.C.L.Durham, 1886. F.R.A.S., 1867; F.R.S., 1884.

Was Lecturer on Natur. Philosophy and Professor of

Mechanical and Experimental Physics in the Andersonian University of Glasgow, 1866–71; and Professor of Physics and Experimental Philosophy in the Univers. of Durham College of Science, Newcastle-on-Tyne, 1871–86, and Honorary Professor of that Institute since 1886.

Writer of the Reports to the British Association on Luminous Meteors and on the Thermal Conductivities of Rocks, 1861–81; and of various papers relating to Meteors, Meteorology, and Physics in Societies' Proceedings and in Scientific Journals since 1860. (*Ipse scripsit*, September 14, 1893. Signed, A. S. Herschel.)

He died on June 18, 1907, and was buried in Upton Churchyard next his sister, Isabella Herschel.

The Sun-spots of 1907 *March.*

March showed a distinct falling-off in the solar activity as compared with January and February, but its record was not without some features of interest. During the first four or five days of the month the Sun's surface appeared strewn with numerous groups of spots, none large or remarkable in themselves, but indicating by their number and diffusion a general condition of agitation. On March 3 Groups 6135 and 6136, together making up a fine stream, began to show themselves at the eastern limb of the Sun, and they were followed by a yet finer stream on March 5, consisting of Groups 6139 and 6140. These two streams, with some smaller groups in the same latitude, made up a striking procession in the southern hemisphere, and remained in sight until March 17, when they passed off at the western limb. After this there was very little to call for notice. A pretty but by no means large stream—Group 6150—ran its course in the northern hemisphere from March 16 to March 25, and another—Group 6155—in the southern from March 21 to March 29; but these were the only two groups of any importance until, on the last day of the month, two new groups—Groups 6158 and 6159—were seen in the northern hemisphere just coming into view at the east limb. Some theoretical interest attaches to Group 6154, a small round spot observed from March 17 to March 21, and now in its third apparition, as it was almost exactly on the solar equator, where spots do not frequently form except during the decline of the Sun's activity.

Rotation 715 began 1907 March $7^{d}\cdot65$.

Notes on the Principal Spot-Groups.

Group 6134.—A large regular spot. Area on March 6 about 200.

Group 6136.—Return of Group 6103. Third apparition. A large regular spot, closely following Group 6135. The group changed much after March 8. Area on March 8 about 360.

Spot-Groups in 1907 March.

No. of roup.	Observed.		Dura-tion.	Date for C.M.	Longi-tude.	Lati-tude.	Remarks.
	From	To					
	1907.	1907.	days.	d	°	°	
6132	Mar. 1	Mar. 1	1	Mar. 3·8	51	−20	Small spots *f.* Gr. 6129.
3	2	3	2	6·4	16	+17	Very small spot.
4	2	13	12	8·0	356	+13	Large regular spot.
5	3	13	11	8·9	343	−15	Straight stream.
6	3	15	13	9·4	337	−14	**Return.** Regular spot.
7	4	8	5	4·2	46	− 9	Straight stream.
8	4	11	8	6·5	15	− 5	Fine cluster.
9	5	12	8	11·0	316	−17	Regular spot.
6140	5	17	13	11·5	309	−16	**Return.** Composite spot.
1	7	8	2	3·9	50	+12	Few very small spots.
2	7	13	7	10·9	317	−10	Short stream.
3	8	8	1	8·6	348	+15	Very small spot.
4	8	8	1	9·2	340	+15	Very small spot.
5	12	19	8	15·8	253	−13	Small regular spot.
6	14	16	3	12·1	302	−23	Short stream.
7	16	16	1	11·6	308	−25	Few small spots.
8	16	18	3	12·5	296	−20	Small irregular stream.
9	16	16	1	17·3	232	+10	Close pair of small spots.
6150	16	25	10	19·6	203	+ 9	Irregular stream.
1	16	16	1	21·1	182	+ 9	**Return.** Small spot.
2	16	23	8	21·3	180	− 9	Regular spot.
3	17	18	2	21·9	172	−16	**Return.** Very small spot.
4	17	21	5	22·3	167	+ 1	**Return.** Very small spot.
5	21	29	9	26·1	117	−19	Small stream.
6	30	Apr. 8	10	Apr. 4·3	356	+12	**Return.** Regular spot.
7	31	3	4	Mar. 30·7	56	−18	Straight stream.
8	31	10	11	Apr. 4·9	348	+16	Straight stream.
9	31	12	13	6·6	325	+13	Very fine stream.
6160	31	3	4	6·8	323	−11	Pair of small spots.

Group 6137.—A straight stream suddenly appearing *nf.* Group 6129, and quickly diminishing in size. Area on March 4 about 120.

Group 6138.—One or two small spots when first seen. The group suddenly enlarged by March 8 and became a fine cluster. Area on March 9 about 500.

Group 6139.—A regular spot crossed by a bright bridge. Also a few small attendants. Area on March 8 about 100.

Group 6140.—Return of Group 6108. Third apparition. A very fine composite spot, *f.* Group 6139. Also several small attendants. Area on March 9 about 800.

Group 6150.—A few small spots in a scattered stream. The group gradually increased in size as it crossed the disc. Area on March 20 about 100.

Group 6151.—Return of Group 6119. A very small spot. Area about 5.

Group 6153.—Return of Group 6118. A very small spot. Area on March 17 about 3.

Group 6154.—Return of Group 6117. Third apparition. A very small spot. Area on March 17 about 5.

Group 6155.—A small stream slowly increasing in size. The leader became a regular spot by March 25. Area on March 25 about 120.

Group 6156.—Return of Group 6134. Regular spot. Area on March 31 about 100.

Group 6158.—A few small spots at first, *f.* Group 6156. Later the group developed into a fine straight stream. Area on April 5 about 240.

Group 6159.—A very fine stream. Area on April 5 about 1000.

E. Walter Maunder.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The First Comet of 1742.

Gentlemen,—

The remark of Prof. Berberich (*Ast. Nach.* No. 4174) that the elements of Comet *b* 1907 (which was first discovered by Mr. Grigg in New Zealand on the 9th of April) have a great similarity to those of Comet 1742 I., recalls attention to the latter, as the bodies may be identical. The definitive determination of the orbit of the comet of 1742 was made by Dr. Cohn, of Strassburg, and is published in *Ast. Nach.* Nos. 4072-3 (vol. 170. col. 245), together with a history of the observations and a comparison of them with his elements. He afterwards made, at the instance of Prof. Kreutz, a slight correction (*Ast. Nach.* No. 4111, vol. 172. col. 105), but it is of a very small amount, affecting only the perihelion distance and time of perihelion passage.

The comet was first seen at the Cape of Good Hope on the 5th of February, and was afterwards noticed by two Dutch navigators on the 27th and 28th of the same month. The first person to see it in Europe was an Irishman named Grant, who saw it at Paris on the 2nd of March. The first observation obtained was by Maraldi on the morning of the 5th of that month, when it was also observed by Lacaille and Lemonnier. The perihelion passage had occurred on the 8th of February; when the comet was noticed by Grant it was equal in brightness to Jupiter, with a tail about 5° in length, which increased until the 13th of March, and afterwards diminished, ceasing to be visible on the 27th. Observations were continued for about two months, the last recorded being by Maraldi on the 6th of May. It is to be regretted that very few observations were obtained in England. This is to a great extent due to the fact that Bradley had only recently been appointed to succeed Halley at Greenwich, who had died on the 14th of January, and very much of Bradley's time in the early part of 1742 was taken up in getting the instruments in his hands into proper order and adjustment. It appears, however, from Rigaud's Memoir (p. lxii), prefixed to his 'Miscellaneous Works,' that the first account he had of the comet was that it had been seen at Melksham, in Wiltshire, on the 20th of February, after which he made observations of its distance from the stars. Only one of these seems to have been preserved, which is given in Rigaud, p. 371, who says that it "is not dated, but it appears from some computations on the same paper to have been made on the morning of the 24th of February, 1742." In comparing this with the dates of the French observations, it must be remembered that Old Style was still used in England, and that the 24th of February corresponded to the 7th of March New Style, and that the Melksham perception of the comet was one day later than that of Grant in Paris.

Rigaud gives (Memoir, p. xliii) a set of elements which had, he says, been found in Bradley's handwriting, and it is to be presumed are his own.

Dr. Cohn mentions the above observation by Bradley, and regrets that he cannot use it in his investigation because the exact time is not given.

Pingré gives a number of determinations of the orbit, one of which, by Euler, was elliptic with a period of about 42 years, but that geometer himself thought that the observations were not sufficiently exact to warrant such a conclusion, and believed that the orbit was probably parabolic. On this point Dr. Cohn says:—"Wir haben den Fall nicht weiter untersucht, ob ein elliptisches Elementsystem einen oder mehrere Normalörter besser dargestellt hätte als es die Parabel tut." But whilst thus expressing himself doubtfully as to whether the orbit was really elliptical, he utterly rejects Zanotti's suggestion that the comet was identical with one seen in 1702.

However, it is not without interest that the period resulting from one of Euler's determinations is almost exactly a quarter of the period between the appearance of the one discovered by Grigg and afterwards by Mellish this year and the first comet of 1742.

Yours faithfully,

Blackheath, 1907, June 3. W. T. LYNN.

Comet Grigg, 1907, April.

GENTLEMEN,—

The first intimation I received of the discovery of this comet was by an announcement from the Sydney Observatory in the *Sydney Morning Herald* of the 1st instant and also by a private letter from Mr. C. J. Merfield on the same date. Although I am credibly informed that the discovery was originally, though in a very imperfect way, cabled by the discoverer to Melbourne and that private letters containing rough positions were subsequently forwarded by him to the State Observatories, I was perfectly in the dark on the subject till the date above named. Mr. Merfield furnished me with a statement of the orbit-elements and also an ephemeris extending from May 1 to 17. It was then, however, too late to attempt to pick up the comet, as it must have been faint, was in high north declination, and was subject to a bright Moon. It is not likely that any observations will be contributed from Australia on this occasion. I cannot understand why the information was suppressed so long at the State Observatories, or why the discoverer himself, who is a correspondent of mine and who knows that nearly all the comet observations in Australia for many years past have devolved upon me, ignores the existence of my Observatory. His first comet, discovered in 1902, was observed only by himself, and roughly too, because he failed to communicate the discovery in time to those who were better able to make observations. His second comet, that of 1903, nearly passed unobserved for the same reason. Fortunately, however, it was picked up at this Observatory, but it was observed at one other station only, namely, the Royal Observatory, Cape of Good Hope. I may add that in former years I used to receive copies of Kiel cable-messages directly from Melbourne, but now I have to depend at second hand on Sydney. I have just been informed, through the courtesy of the Government Astronomer at Sydney, that the new comet was found independently by Mellish at Madison, U.S.A., on April 14, so I trust it has been well observed in the northern hemisphere.

Windsor, N. S. Wales,
1907, May 15.

Yours faithfully,
JOHN TEBBUTT.

Jupiter's Family of Comets.

GENTLEMEN,—

It is a remarkable fact, which has several times been commented upon by myself and others, that the comets of short period move in orbits the great majority of which have their perihelia between 270° and 90°.

Of 34 comets (including not only the better known objects but those less accurately determined from only one return), no less than 26 have their perihelia between the longitudes stated, while 8 only are placed between 90° and 270°. Mere accidental grouping could scarcely introduce such a wide difference, and the fact mentioned is probably traceable to some physical cause strongly tending to this concentration on one side of the Sun.

The various comets assorted according to the longitudes of their perihelia are as follow :—

Between 270° and 90°.

Tempel (1873).
Tempel-Swift (1869–1880).
Pons-Winnecke (1819–1858).
E. Swift (1894).
Finlay (1886).
D'Arrest (1851).
Wolf (1884).
Holmes (1892).
Brooks (1889).
Faye (1843).
Blanpain (1819).
Barnard (1884).
De Vico (1844).

Lexell (1770).
Pigott (1783).
Spitaler (1890).
Barnard (1892).
Perrine (1896).
Giacobini (1896).
Giacobini (1900).
Borrelly (1905).
L. Swift (1895).
Denning (1881).
L. Swift (1889).
Kopff (1906).
Metcalf (1906).

Between 90° and 270°.

Encke (1786–1819).
Brorsen (1846).
Tempel (1867).
Biela (1826).

Helfenrieder (1766).
Brooks (1886).
Tuttle (1858).
Denning (1894).

The average perihelion distance is nearly the same as the interval separating the orbit of Mars (1·5) from the Sun.

It is worthy of particular mention that the greatest number of meteors are encountered by the Earth when moving from 270° to 90° longitude. These are both swift and slow meteors in apparently fair proportion, omitting the members of the great Perseid shower. As the periodic comets belonging to the Jovian group move in direct orbits, meteors derived from them must overtake the Earth and appear to move slowly or somewhat slowly according to the position of the individual radiants.

There would appear to be a clustering both of meteors and of the perihelia of short-period comets between 270° and 90°. But the fact must not be lost sight of that the known cometary orbits, even at the point of nearest approach to the Earth, are presumably too distant to occasion visible meteoric showers: Biela's, however, furnishes a great exception. But there are certainly a large number of periodical comets still unknown to us, and the débris of attenuated comets may be profusely distributed in the longitude mentioned, though the material (like that of the familiar case of Biela) has ceased to be visible under the usual cometary form.

Yours faithfully,
Bristol, 1907, June 7. W. F. Denning.

OBSERVATORIES.

Royal Observatory, Greenwich.—Visitation Day this year happened on the second Saturday in June, because it would have been difficult for several members of the Board, who had been at Vienna at the International Association of Academies, to have attended a week earlier. The following details are extracted from the Astronomer Royal's Report.

Transit-Circle.—The number of transits observed during the year was 7704, of Circle observations 7311. The objects now being observed, besides the Sun, Moon, Planets and Fundamental stars, are the Reference stars for the Oxford Astrographic Zone. The apparent correction to zenith-point for discordance between observations of the nadir and stars by reflexion for 1906 was found to be $-0''\cdot38$. The correction for the R−D discordance was found to be $+0''\cdot089+0''\cdot472 \sin$ z.d. The mean error of the Moon's tabular place (computed from Hansen's tables with Newcomb's corrections) is $-0^{s}\cdot412$ in R.A. deduced from 94 observations. The correction to the tabular obliquity of the ecliptic is not given, possibly because the corrections for variation of latitude had not been received from Prof. Albrecht.

The Altazimuth.—This instrument has been used as a reversible transit-circle in the meridian in four positions during the year, observations being made of the Sun, Moon, Planets, and Fundamental stars in the National Ephemerides and in Newcomb's Catalogue. It has also been used for extra-meridian observations of the Moon during the first and last quarters, observations of the crater Mösting A being made whenever possible.

With the *Reflex Zenith Tube*, 901 double observations and 34 single observations have been obtained during the year, the number of different stars observed being 85. The programme of work with the instrument has been extended, so that now the full scheme of observation of two principal groups of stars and a supplementary group to strengthen the overlap of the two principal groups is being carried out.

With the Equatorials, twenty-four disappearances and three re-appearances of stars occulted by the Moon have been observed by one or more observers. Bad weather and some necessary repairs reduced the number of nights on which observations were made with the 28-inch refractor to 86 as compared with 105 in the previous year. κ Pegasi, whose period is 11·4 years, has now been followed by this instrument during a whole period and measures made of it on 129 nights distributed round the orbit. The 26-inch refractor on the Thompson Equatorial has been used to photograph Neptune and its satellite, and with the 30-inch reflector good series of photographs of Jupiter's sixth and seventh satellites, of minor planets and comets, together with a few photographs of nebulæ and star-clusters, have been obtained.

The printing of the measures of the Astrographic Catalogue is completed and the computation of plate constants is proceeding.

The solar activity as shown in the numbers and areas of sun-spots showed some remarkable fluctuations during the year. A great revival began at the end of May and culminated in the months of July and August. October was a quiet month. A steady recovery set in in November, which continued until March in the present year, and some of the largest spot-groups of the present sun-spot maximum were observed during December, January, and February.

The mean temperature for the year 1906 was 50°·5, or 0°·9 above the average. The highest temperature in the shade was 94°·3 on August 31. The lowest temperature of the air recorded in the year was 19°·8 on December 30. There were 58 days in the winter on which the temperature fell below 32°·0, which is two more than the average number, and it also fell below 32°·0 on 1906 May 18. The number of hours of bright sunshine recorded during the twelve months ending 1907 April 30 was 1687 out of 4457 hours during which the Sun was above the horizon. The rainfall for the year ending 1907 April 30 was 23·86 inches, being 0·26 inch less than the average of the 65 years 1841–1905. The number of rainy days was 148.

The Astronomer Royal closes his Report with some remarks on the L.C.C. electric generating station. Though the tremors which are undoubtedly caused by the station can be masked by the use of a very thin film of mercury, it is a question whether they do not cause the telescopes to oscillate to such an extent as to interfere with the most delicate work for which they are capable. There is also an insidious danger to be feared from the discharge of heated gases in the neighbourhood of the Greenwich meridian. The effect on the accuracy of observations of stars near the north horizon may only become apparent when it is found that the results are untrustworthy from this cause.

Perth, West Australia.—This is mainly a meteorological observatory as yet, or rather the results already published are wholly meteorological, though it was intended to be astronomical as well. In his Report for 1905, Mr. W. E. Cooke, the Government Astronomer, explains his position with respect to the latter branch of his work, in the hope evidently of getting more financial help.

It is no doubt known that when the Astrographic Chart and Catalogue work was begun, three observatories of South America each offered to take a zone, but for various reasons did not at once proceed with the work. Santiago, has done so later, but the other two zones have now been allotted elsewhere, one of them, 31° to 41° S. declination, being undertaken by the Perth Observatory. The total number of fields in the zone is 1375, and of 1230 of these the catalogue plates have been taken; but the staff of the Observatory is not sufficient to measure and reduce these plates, which are beginning to deteriorate. Mr. Cooke appeals to the Colonial Secretary to make some provision for carrying on this work, and suggests the employment of ladies, as at Melbourne and Sydney. Beyond the measuring and reducing of the plates, it is necessary that the standard stars should be observed with the transit-circle, and this work has been begun according to a scheme laid down by Mr. Cooke in a paper in the 'Monthly Notices R. A. S.' for March 1906, the gist of which is that this Observatory should take charge of the portion of the sky between 31° and 41° S. dec., and in time furnish all observations of precision within those limits which will be required for ordinary astronomical purposes.

PUBLICATIONS.

Eros Circular No. 12.—The international work of determining the solar parallax by observation of the planet Eros was initiated by the distribution of a few printed pages to which the word "Circular" was not inappropriate. These Circulars have grown, and the concluding number of the series, which has now arrived, takes the form of a good-sized volume of 150 pages. It contains the printed observations of stars and the planet made at various observatories, which it is not necessary to mention in detail, although reference may be made to Prof. Donner's introduction to the Helsingfors section, which shows the probable error of the deduced position of a star of magnitude 9·5 to be ±0″·183 in R.A. and ±0″·160 in Dec., which are rather larger than the similar quantities in the Helsingfors Astrographic Catalogue, and also to Mr. Hinks's note to the Cambridge and Oxford observations, which he has corrected for aberration after the plan suggested by Mr. Cowell in this Magazine for 1900 December; but as this is the closing of the work, so far as the Paris Observatory is concerned—and here one cannot help pondering on the large and generous share of the labour and of the expense that has been undertaken by M. Lœwy and the French Government,—in the introduction M. Lœwy makes some remarks by way of summary. Fifty-eight observatories took part in the work, and the total number of observations is thus tabulated:—

Eye Observations.

Number of reference stars	671
Meridian observations of these in R.A.	19,200
„ „ „ Dec.	16,198
Observations of Eros in R.A.	3,752
„ „ Dec.	2,890

Photographic Observations.

Number of plates	1,443
„ reference stars	671
„ observations of reference stars	16,201
„ comparison stars	962
„ observations of comparison stars ..	17,129
„ „ Eros	1,310

M. Lœwy adds:—"Photographic measures of high precision have been begun at a quite recent date, and necessarily the plan adopted more than fifteen years ago for the Astrographic Catalogue and afterwards applied to the determination of the solar parallax suffers from certain imperfections due to want of experience at the beginning of these undertakings. The precautions necessary to guard against such difficulties have been published by me in three memoirs. In these will be found the explanation of certain small systematic errors, and especially those brought to light by Mr. Hinks in the course of his researches based on the data of these Circulars.

"In spite of these imperfections, the end that the Paris Conference of 1900 had in view has been completely attained. The vast amount of material collected from the co-operating observatories will lead to a value of the solar parallax much superior in precision to those which have gone before."

NOTES.

Comet Notes.—Two new comets were discovered during June. 1907 *c* by M. Giacobini at Nice on June 1, when it was of the 13th magnitude. The following elements are by Dr. Strömgren from observations on June 1, 5, 8:—

T	1907 May 31·17 G.M.T.	
ω	39° 35′·1	1907·0.
☊	160 52·2	
i	14 51·0	
log q......	0·0924	

The brightness is diminishing slowly, being ⅔ of that at discovery on July 9.

Ephemeris for Berlin Midnight.

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
July 1....	12	25	52	16	49	July 13....	13	15	13	12	12
5....	12	42	44	15	20	17....	13	30	43	10	37
9....	12	59	12	13	47	21....	13	45	39	9	3

1907 *d* was discovered by Mr. Daniel at Princetown, June 9·804 G.M.T., R.A. 23^h 48^m 32^s, S. Dec. 1° 8′, Daily Motion $+2^m$ 16^s, N. 14′; it was of the 11th magnitude, but is brightening fairly rapidly, and may probably be visible to the naked eye in August.

Elements by Dr. Strömgren from observations on
June 13, 15, 16.

T	1907 Sept. 2·01 Berlin M.T.	
ω	241° 59′	1907·0.
☊	143 42	
i	6 15	
log q	0·1144	

Ephemeris for Berlin Midnight.

	R.A. h m s	N. Dec. ° ′		R.A. h m s	N. Dec. ° ′
July 2....	0 57 25	4 29	July 14....	1 40 19	7 34
6....	1 11 14	5 31	18....	1 55 35	8 34
10....	1 25 32	6 33	22....	2 11 20	9 34

On July 22 the theoretical brightness will be 3 times that at discovery. On June 19 it was estimated to be of the 8th magnitude.

The small inclination leads one to anticipate an elliptic orbit as not improbable; there is no very close agreement with the elements of any known comet, but a distant resemblance to that of 1770 I.

The following elements of 1907 *a* are by Misses Einarson, Glancy, and Joy, from observations on March 9, 13, April 3, 9:—

T	1907 March 19·229 G.M.T.	
ω	317° 10′ 7″	1907·0.
☊	97 11 7	
i	141 39 16	
q	2·05160	

A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been discovered:—ZO, by M. Liapin, April 5 (this planet is considered doubtful); ZP, ZQ, ZR (mags. 13, 13, 14), by Dr. Metcalf, at Taunton, Mass., May 4, 5, 11; ZS (mag. 12), by Herr Lohnert, at Heidelberg, June 9.

The following have received names:—398 Admete, 410 Chloris, 411 Xanthe, 414 Liriope, 426 Hippo, 427 Galene, 429 Lotis, 430 Hybris, 431 Nephele, 437 Rhodia, 438 Zeuxo, 441 Bathilde, 453 Tea (!!), 497 Iva, 506 Marion, 508 Princetonia, 517 Edith, 518 Halawe, 519 Sylvania, 523 Ada, 533 Sara, 534 Nassovia, 535 Montague, 537 Pauly.

There is still a third minor planet of the Jupiter group, viz. VY, discovered in 1906, for which Herr Vladimir Heinrich has deduced the following elements:—

Epoch	1906 Oct. 21·5 Berlin M.T.	
M	41° 31′ 55″	
ω	297 28 37	1906·0.
☊	43 21 39	
i	22 16 47	
ϕ	8 42 41	
μ	300″·145	
log a	0·715117	

Dr. Strömgren notes that TG and XM are respectively 11° 58′ and 18° 54′ distant from the equilateral point, on the side of Jupiter whose longitude is greater than his, while VY is distant only 5° 57′ from the equilateral point on the opposite side of Jupiter.

Prof. Charlier's conjecture that there might be other bodies of this kind is thus confirmed; he notes that a pupil of his, Herr J. F. Linders, is investigating the stability of these orbits, which he does not consider would be endangered by the action of other planets.

Dr. Strömgren writes to say that the note on XM in the last Number did not quite accurately express his views.

The following is a translation of the greater part of his letter:—

"I did not say (and did not mean) that XM had nothing to do with Lagrange's Proposition. I said that the theoretical results hitherto obtained for the case where a third body exists at a point not exactly coinciding with the equilateral point, but still not too widely distant from it, are only a first approximation, which in such a case as XM (distant 19° from the equilateral point) would probably not at all resemble the true motion.

"Therefore I mean only that the periodic solutions referred to, where only the first power of the distance from the equilateral point is taken into account, probably cannot serve as the foundation of the solution, a conclusion which was also arrived at by Prof. Charlier in *Ast. Nach.* 4181.

"Quite distinct, however, is the question whether a solution can be arrived at which is independent of this development in powers of the distance, and I await with great interest the results promised by Prof. Charlier in this connection.

"A further distinct question is what influence the other large planets may have on the motion of such a body as we are considering. There is no doubt that it is possible for a small planet to remain for a very long time in the neighbourhood of the equilateral point; but, according to my views, it must provisionally be considered an open question what influence the other bodies of the solar system would have upon the *stability* of its motion."

A. C. D. C.

OBITUARY.—The death of Prof. ALEXANDER HERSCHEL is mentioned on another page.

We also have to record the death of Dr. E. J. ROUTH, F.R.S., of Cambridge, which happened on June 7 last. Dr. Routh can hardly be claimed as an astronomer, but in the early part of his career the post of Chief Assistant in the Royal Observatory, Greenwich, was offered him, and he spent a short time at Greenwich in making himself acquainted with the work. But the routine and the handling of instruments was not to his liking, and he preferred to adopt the line of pure mathematics, in teaching which he became so famous. Dr. Routh married the eldest daughter of Sir George Airy in 1864.

WE hear also of the death of M. TRÉPIED, Director of the Algiers Observatory. This is a severe addition to the many losses that French astronomy has suffered lately.

It is announced that Father Carl Braun, Director of the Observatory in Kalocsa, Hungary, died on June 3 last, aged 77.

Saturn.—This planet is now at the position in its orbit when the rings are invisible. By reference to the *N. A.* it will be seen that the Sun seen from Saturn is at present north of the rings, whilst the Earth is south, so that we are looking at the dark side of the rings, which are therefore probably invisible. On July 26 the plane of the rings will pass through the Sun and their comparatively thin edge will then be illuminated, and from that date the Sun and the Earth will both be south of the ring-plane, so that the rings will be seen as a needle of light through the ball of the planet. On October 4 the Earth and Sun will again be on opposite sides of the ring-plane and the appendages will again be invisible.

At this time, too, the satellites of Saturn are seen to be eclipsed and to transit the disk of the planet. A list of these phenomena is given by Dr. Hermann Struve in the 'Publications of the Astronomical Society of the Pacific' for June. During July the following eclipses of Titan will take place :—Dis. $8^d\ 23^h\ 33^m$ G.M.T., Re. $9^d\ 5^h\ 26^m$; Dis. $24^d\ 22^h\ 40^m$, Re. $25^d\ 4^h\ 37^m$. There will be a transit of the same satellite on July $17^d\ 13^h\ 48^m$, and of its shadow 5½ hours earlier. The times of disappearance at eclipse of the inner satellites can be inferred from their times of eastern elongation given in the *Nautical Almanac*. Mimas disappears about $7^h{\cdot}3$, Enceladus $10^h{\cdot}0$, Tethys $13^h{\cdot}6$, Dione $19^h{\cdot}0$, Rhea $30^h{\cdot}4$, before each elongation.

Mr. Denning points out, in *Nature* of June 20, that as the rings are now turned edgeways to us, the belts in both hemispheres can be observed to advantage, and the opportunity should be taken to examine the surface of the planet for surface-markings, and to ascertain their rates of velocity in different latitudes, as has been done for Jupiter.

There was a Special Evening Meeting at Burlington House on June 26 at 8.30, when Prof. Hale gave a lecture on "The Opportunities for Astronomical Work with Inexpensive Apparatus." Members of the British Astronomical Society were invited, and there was an attendance estimated at about a hundred. Mr. W. H. Maw occupied the Chair in the absence of Mr. Newall, who is out of England. The lecture was fully reported, and will be published verbatim, so that it may be sufficient to say here that Prof. Hale explained several lines of solar research that might be undertaken by observers possessed of an Evershed spectroscope on a 4-inch or 6-inch equatorial, or of a modest spectroheliograph, which might be made cheaply.

Mars will be in opposition on July 6, and, as usually happens at such times, paragraphs have appeared in the less reliable newspapers to the effect that spontaneous signals are being received at the wireless telegraphy station at Cape Clear which possibly have their origin in Mars. *Nature* publishes a note from Mr. Marconi, who disclaims all knowledge of such signals; in fact he says there is no wireless telegraph station at Cape Clear.

Leicester is to be the meeting-place of the British Association this year, and on July 31 Sir David Gill, K.C.B., F.R.S., this year's President, will deliver his inaugural address.

From an Oxford Note-Book.

Experience of two international conferences, differing greatly in character, in consecutive weeks, suggests some reflections on the true inwardness of these gatherings. The first was a meeting of people with a definite and real common interest; they might be astronomers or physicists or meteorologists or chemists; they might hail from any quarter of the globe; they might have never met before; but they were all keen about the Sun and its problems, and they generally knew one another by name or by work, and were eager to improve the acquaintance in the future. Differences of race, profession, or religion were no hindrance to the formation of a real Solar Union with natural ties. The International Association of Academies has much greater difficulties to contend with in struggling into existence. The men who meet together are all devoted to the advancement of learning, but they have been at work in such widely different fields that their very names are often strange to one another. They are concerned, to a large extent, with the sordid problems of obtaining aids to research rather than with the fascinating researches themselves, or with the diplomatic questions of bringing coy governments into friendly relationship, and on such topics there is room for wide differences of opinion.

That in face of these difficulties everything went so smoothly at the recent meeting is immensely creditable to the Vienna Academy, which on this occasion played the part of host, and to the members of the Executive Committee, who had spent so much time and care on the preparation of the programme. The work was all put into excellent shape, doubtful or embarrassing questions being excluded, and only matters well within the range of practical politics admitted. The general body were, to do them justice, quite as ready as the Committee to act in this sense, and two of their earliest decisions were to reject at the outset proposals to consider the selection of an "auxiliary language" (Volapuk, Esperanto, or what not) or the arrangement of a symbolic nomenclature for machinery. At first sight these questions would seem eminently suitable for discussion by an international assembly, but the discussions might be long and laborious ere any conclusions were reached, and the Association was no doubt wise in determining not to undertake them at present.

Of their constructive decisions, two at least were of astronomical importance. They decided to appoint an international committee to consider lunar nomenclature, and the following were nominated as members, with power to add to their number:—Lœwy (Chairman), Franz, Newcomb, Saunder, Turner, Weiss. Secondly, they gave their formal blessing to the International Union for Solar Research, and intimated that they would gladly receive a triennial report from it, and would nominate (through the leading Academy for the time being) one of the members of the Committee. And, besides these formal decisions, there were other items of interest to us. We learnt that the apparatus of Baron Eötvös for measuring gravity is so promising that the Hungarian Government have voted £2500 a year for three years for experiments with it. This is good news indeed, and, in hopeful vein, the Association determined to appeal to the Italian Government for further experiments in the neighbourhood of volcanoes such as Vesuvius. We heard, too, that the South African Survey is approaching the limits of British territory, and the Association forthwith struck an attitude of expectancy, listening, so to speak, for an utterance from Germany as regards the continuation through German territory. We heard further that difficulties had been smoothed away from the path of the International Seismological Association; that it was hoped to carry a magnetic survey round a parallel of latitude, with supplementary observations; and to commence solar work in Austria if the Government could be satisfied of the value of their mountain-sites. These matters from the domain of "Cosmical Physics" will give a general idea of the work of the Association.

But there was also its play, and some most entertaining games had been arranged for it, presumably in deference to the view so eloquently expressed by M. Darboux that "Science is always young." [This was his explanation of the fact that on the medal struck by the French Government to commemorate the first meeting of the Association at Paris in 1901, those assembled in conference were pictured as young girls, a representation only in distant accord, as he remarked, with the appearance of the company round him.] There was a delightful expedition to the Semmering, where we lunched in full view of snow-topped mountains and meditated on possible sites for a solar observatory. Prof. Hale went so far as to impound a small telescope (intended merely for regarding the surface of the nearest planet) and to point it to the Sun in order to pick up any possible hints about atmospheric effects. This instrument also gave some other hints; it only became available on putting a penny in the slot, and closed automatically after a suitable interval. Now supposing that visitors to astronomical observatories were to be laid under contribution to the funds by —— (?); but the suggestion is not a very exhilarating one.

Then we saw a wonderful fairy castle. It had been knocked down by the Swedes in the 30 years' war, but it had been recently built up again far more beautiful than ever, and just as old. There were the old slits to shoot through and the old battlements to shoot from, and even an old catapult to shoot with, and quaint old furniture inside and thousands of old prints and old books; and horns saluted us when we entered, as though we had returned from hunting the deer as of old; and we looked down the old well, and spent a few moments in the old chapel. Some things were unavoidably modern, such as the chapel organ and the beautiful music we heard from it, and the clock, with its motto "Wer rastet, rostet," reminding us that we must not dwell for ever in the past. And there was in the chapel the flag of the Arctic expedition which our host, the Graf von Wilczek, had organized, and which had discovered Franz Josef Land. But these touches of modernity only threw into stronger relief the wonderful glimpse of the past which our kindly courtly host had provided for our fascinated gaze; and we could all sympathize with the Swedish delegate who was fain to enter a claim for forgiveness by pointing out that if his countrymen had *not* knocked down the castle it could never have been thus built up again.

We had the great honour of being presented to the Austrian Emperor in the evening of the same day. He spoke a few words in turn to each of the 60 or 70 delegates assembled, and the ease and vigour with which he performed this tiring ceremony made it difficult for us to realize that he had been Emperor since 1848, to say nothing of the events of that long reign. Not content with this great consideration, he invited the delegates to the opera on the following evening. There are moments of splendour which one can dwell upon with permanent satisfaction, and those moments of attendance at the opera in Vienna on the Emperor's invitation will, I imagine, take an honourable place of the kind in the memories of all those who enjoyed them.

Our President, the veteran geologist Suess, entertained us at dinner on the first day of meeting. Among the influences which helped to unify the gathering of so many different nationalities and interests, it would be difficult to overestimate the personality of the President. It must have been a tiring week for him, but he gave all his energies unsparingly and ungrudgingly, and for a man with a great work on hand the sacrifice was no small one.

Of the private hospitality, gracefully extended, this is not the place to speak. For us of English speech it was rendered particularly pleasant by the widespread knowledge of our language and customs, which surprised us in Vienna at all turns. Even the vehicles drive on the left side of the road. But perhaps we are wrong in the inference that special attention is paid to English. One fair neighbour whom I had the good fortune to meet seemed to be equally at home in several languages, though she professed herself puzzled by the conversation going on opposite us. Recognizing a friend in one of the interlocutors, I asked him, for her benefit, what language it was: "Swedish and Danish" was the reply. "I can understand Danish, though I cannot talk it, and my neighbour is similarly situated with regard to Swedish."

There were, of course, some misguided people who sought out unofficial entertainments. Two of these came back with stories of a kinematograph expedition to the Moon which gave a colour of astronomical interest to their enterprise. But what they said does not account for the subsequent enquiry from a high geodetic authority whether the "Moulin rouge" was "wieder los," nor for the discovery of a programme containing references to "Cake walk dancus" and an "English daming girl." It must be people of this kind who celebrate Empire Day, according to the foreign newspapers, by singing "Home Rule Britannia."

Glancing back over the above paragraphs, I see that a project to which an astronomical character was assigned by at least one authority has been overlooked. It was judged desirable to test the stability of the soil by levelling operations at intervals of a few years, not only in the neighbourhood of earthquake-centres, but elsewhere. The conversation was of course conducted in German, and the following account of it appeared next morning in the daily papers:—

> Es wurde noch die Resolution angenommen, dass die Messung der Stabilität der Sonne mittelst genauer Nivellierungen nach längeren Intervallen wünschenswert sei, und zwar nicht nur in den Gebirgsketten, welche den Erdbeben unterworfen sind, sondern auch in allen anderen Regionen.

I must not forget our visits to the two observatories at Vienna. Prof. Weiss and his family (which includes the charming originals of several minor planets) welcomed us to the national observatory in a manner so hospitable that we saw but little of the observatory itself. But the fame of it and of Palisa's discoveries are well established. Will it be credited that it was actually in Vienna that a depraved human being spoke of "comets and small planets and other vermin of the heavens?" Can envy, hatred, malice and all uncharitableness go further than this?

The von Kuffner Observatory is beautifully placed at Ottakring, a few miles from the city, and bears the legend—

Dem Guten its's zu gonnen
Zu ruhen wenn die Nacht begonnen
Doch bist du Astronom
Bei Tag zum schlafen kom̄.

Whether Mr. de Ball sleeps by day did not appear, but, judging from the amount of work he turns out single-handed, he cannot sleep much by night. He has had excellent assistants in the past, but for the moment the quarters (for two assistants) are unoccupied. If anyone is looking for a beautiful spot to work in, with good instruments, a free choice of programme, and a modest competence, he cannot do better than apply at Ottakring.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. AUGUST, 1907. No. 386.

MEETING OF THE BRITISH ASSOCIATION AT LEICESTER.

Address by the President, Sir DAVID GILL, K.C.B., LL.D., D.Sc., F.R.S., Hon.F.R.S.E., &c., 1907, *July* 31.

ONE is tempted to take advantage of the wide publicity given to words from this Chair to speak at large in the cause of science, to insist upon the necessity for its wider inclusion in the education of our youth, and the devotion of a larger measure of the public funds in aid of scientific research; to point to the supreme value of science as a means for the culture of those faculties which in man promote that knowledge which is power; and to show how dependent is the progress of a nation upon its scientific attainment.

But in recent years these truths have been prominently brought before the Association from this Chair; they have been exhaustively demonstrated by Sir William Huggins from the Chair of the Royal Society, and now a special guild * exists for their enforcement upon the mind of the nation.

These considerations appear to warrant me in following the healthy custom of so many previous Presidents—viz., of confining their remarks mainly to those departments of science with which the labours of their lives have been chiefly associated †.

The Solar Parallax.

Much progress has been made in the exact measurement of the great fundamental unit of astronomy—the solar parallax.

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* The British Science Guild.

† By the kindness of Sir David Gill, we have been enabled to make this abstract of his Presidential Address. Other sections, omitted for want of space, have for their subjects: *The Science of Measurement, The Great African Arc of Meridian, The Comparative State of Astronomy in the Northern and Southern Hemispheres, Stellar Parallax, Determination of Stellar Motion in the Line of Sight,* and *The Future Course of Research.*

Early in 1877 I ventured to predict * that we should not arrive at any certainty as to the true value of the solar parallax from observations of transits of Venus, but that the modern heliometer applied to the measurement of angular distances between stars and the star-like images of minor planets would yield results of far higher precision.

The results of the observations of the minor planets Iris, Victoria, and Sappho at their favourable oppositions in the years 1888 and 1889, which were made with the co-operation of the chief heliometer and meridian observatories, fully justified this prediction †. The Sun's distance is now almost certainly known within one-thousandth part of its amount. The same series of observations also yielded a very reliable determination of the mass of the Moon.

The more recently discovered planet Eros, which in 1900 approached the Earth within one-third of the mean distance of the Sun, afforded a most unexpected and welcome opportunity for redetermining the solar parallax — an opportunity which was largely taken advantage of by the principal observatories of the northern hemisphere. Unfortunately the high northern declination of the planet prevented its observation at the Cape and other southern observatories. So far as the results have been reduced and published ‡ they give an almost exact accordance with the value of the solar parallax derived from the heliometer observations of the minor planets, Iris, Victoria, and Sappho, in 1888 and 1889.

But in 1931 Eros will approach the Earth within one-sixth part of the Sun's mean distance, and the fault will rest with astronomers of that day if they do not succeed in determining the solar parallax within one ten-thousandth part of its amount.

To some of us who struggled so hard to arrive at a tenth part of this accuracy under the less favourable geometrical conditions that were available before the discovery of Eros, how enviable seems the opportunity!

And yet, if we come to think of it rightly, the true opportunity and the chief responsibility is ours, for *now* and not twenty years hence is the time to begin our preparation; *now* is the time to study the origin of those systematic errors which undoubtedly attach to some of our photographic processes; and then we ought to construct telescopes specially designed for the work. These telescopes should be applied to the charting of the stars near the path which Eros will describe at its opposition in 1931, and the resulting star-co-ordinates derived from the plates photographed by the different telescopes should be rigorously inter-compared. Then, if all the telescopes give identical results for the star-places, we can be certain that they will record without systematic error the position of Eros. If they do not give identical results, the source of the errors must be traced.

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* "The Determination of the Solar Parallax," *The Observatory*, vol. i. p. 280.

† 'Annals of the Cape Observatory,' vol. vi. part 6, p. 29.

‡ 'Monthly Notices R. A. S.,' Hinks, vol. lxiv. p. 725; Christie, vol. lxvii. p. 382.

The planet will describe such a long path in the sky during the opposition of 1931 that it is already time to begin the meridian observations which are necessary to determine the places of the stars that are to be used for determining the constants of the plates. It is desirable, therefore, that some agreement should be come to with respect to selection of these reference-stars, in order that all the principal meridian observatories in the world may take part in observing them.

I venture to suggest that a Congress of Astronomers should assemble in 1908 to consider what steps should be taken with reference to the important opposition of Eros in 1931.

The Stellar Universe.

And now to pass from consideration of the dimensions of our solar system to the study of the stars, or other suns, that surround us.

To the lay mind it is difficult to convey a due appreciation of the value and importance of star-catalogues of precision. As a rule such catalogues have nothing whatever to do with discovery in the ordinary sense of the word, for the existence of the stars which they contain is generally well known beforehand; and yet such catalogues are, in reality, by far the most valuable assets of astronomical research.

If it be desired to demarcate a boundary on the Earth's surface by astronomical methods, or to fix the position of any object in the heavens, it is to the accurate star-catalogue that we must refer for the necessary data. In that case the stars may be said to resemble the trigonometrical points of a survey, and we are only concerned to know from accurate catalogues their positions in the heavens at the epoch of observation. But in another and grander sense the stars are not mere landmarks, for each has its own apparent motion in the heavens which may be due in part to the absolute motion of the star itself in space, or in part to the motion of the solar system by which our point of view of surrounding stars is changed.

If we desire to determine these motions and to ascertain something of the general conditions which produce them, if we would learn something of the dynamical conditions of the universe and something of the velocity and direction of our own solar system through space, it is to the accurate star-catalogues of widely separated epochs that we must turn for a chief part of the requisite data.

The value of a star-catalogue of precision for present purposes of cosmic research varies as the square of its age and the square of its accuracy. We cannot alter the epoch of our observations, but we can increase their value fourfold by doubling their accuracy.

Hence it is that many of our greater astronomers have devoted their lives chiefly to the accumulation of meridian observations of high precision, holding the view that to advance such precision is the most valuable service to science they could undertake, and comforted in their unselfish and laborious work only by the consciousness that they are preparing a solid foundation on which future astronomers may safely raise the superstructure of sound knowledge.

The Constitution of the Universe.

It was not until 1718, when Edmund Halley, afterwards Astronomer Royal of England, read a paper before the Royal Society *, entitled "Considerations on the Change of the Latitudes of Some of the Principal Fixt Stars," that any definite facts were known about the constitution of the universe. In that paper Halley, who had been investigating the precession of the equinoxes, says: "But while I was upon this enquiry I was surprized to find the Latitudes of three of the principal Stars in heaven directly to contradict the supposed greater obliquity of the Ecliptick, which seems confirmed by the Latitudes of most of the rest."

This is the first mention in history of an observed change in the relative position of the so-called fixed stars—the first recognition of what we now call "proper motion."

Tobias Mayer, in 1760, seems to have been the first to recognize that if our Sun, like other stars, has motion in space, that motion must produce apparent motion amongst the surrounding stars; for in a paper to the Göttingen Academy of Sciences he writes:—"If the Sun, and with it the planets and the Earth which we inhabit, tended to move directly towards some point in the heavens, all the stars scattered in that region would seem to gradually move apart from each other, whilst those in the opposite quarter would mutually approach each other. In the same manner one who walks in the forest sees the trees which are before him separate, and those that he leaves behind approach each other." No statement of the matter could be more clear; but Mayer, with the meagre data at his disposal, came to the conclusion that "the motions of the stars are not governed by the above or any other common law, but belong to the stars themselves."

Sir William Herschel, in 1783, made the first attempt to apply, with any measure of success, Mayer's principle to a determination of the direction and amount of the solar motion in space †. He derived, as well as he could from existing data, the proper motions of fourteen stars, and arrived by estimation at the conclusion that the Sun's motion in space is nearly in the direction of the star λ Herculis, and that 80 per cent. of the apparent motions of the fourteen stars in question could be assigned to this common origin.

This conclusion rests in reality upon a very slight basis, but the researches of subsequent astronomers show that it was an amazing accidental approach to truth—indeed, a closer approximation than Herschel's subsequent determinations of 1805 and 1806, which rested on wider and better data *.

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* Phil Trans. 1718, p. 738.

† *Ibid.* 1783, p. 247.

Consider for a moment the conditions of the problem. If all the stars except our Sun were at rest in space, then, in accordance with Mayer's statement, just quoted, all the stars would have apparent motions on great circles of the sphere away from the apex and towards the antapex of the solar motion. That is to say, if the position of each star of which the apparent motion is known was plotted on the surface of a sphere and a line with an arrow-head drawn through each star showing the direction of its motion on the sphere, then it should be possible to find a point on the sphere such that a great circle drawn from this point through any star would coincide with the line of direction of that star's proper motion. The arrow-heads would all point to that intersection of the great circles which is the antapex of the solar motion, and the other point of intersection of the great circles would be the apex, that is to say, the direction of the Sun's motion in space.

But as the apparent stellar motions are small and only determinable with a considerable percentage of error, it would be impossible to find any point on the sphere such that every great circle passing through it and any particular star would in every case be coincident with the observed direction of motion of that star.

Such discordances would, on our original assumption, be due to errors of observation, but in reality much larger discordances will occur, which are due to the fact that the other stars (or suns) have independent motions of their own in space. This at once creates a new difficulty, viz., that of defining an absolute locus in space. The human mind may exhaust itself in the effort, but it can never solve the problem. We can imagine, for example, the position of the Sun at any moment to be defined with reference to any number of surrounding stars, but by no effort of imagination can we devise means of defining the *absolute* position of a body in space without reference to surrounding material objects. If, therefore, the referring objects have unknown motions of their own, the rigour of the definition is lost.

What we call the observed proper motion of a star has three possible sources of origin:—

1. The *parallactic motion*, or the effect of our Sun's motion through space, whereby our point of view of surrounding celestial objects is changed.

2. The *peculiar* or particular motion of the star, *i. e.*, its own *absolute* motion in space.

3. That part of the observed or tabular motion which is due to inevitable error of observation.

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* Phil. Trans. 1805, p. 233; 1806 205.

Secular Parallactic Motion of Stars.

Why should not the apparent parallactic motions of the stars, as produced by the Sun's motion in space, be utilised as a means of determining stellar parallax?

The strength of such determinations, unlike those made by the method of annual parallax, would grow with time. It is true that the process cannot be applied to the determination of the parallax of individual stars, because the peculiar motion of a particular star cannot be separated from that part of its apparent motion which is due to parallactic displacement. But what we specially want is not to ascertain the parallax of the individual star, but the mean parallax of a particular group or class of stars, and for this research the method is specially applicable, provided we may assume that the peculiar motions are distributed at random, so that they have no systematic tendency in any direction; in other words, that the centre of gravity of any extensive group of stars will remain fixed in space.

This assumption is, of course, but a working hypothesis, and one which, from the paper on star-streaming communicated by Prof. Kapteyn of Groningen to the Johannesburg meeting of the Association two years ago, we already know to be inexact *. Kapteyn's results were quite recently confirmed in a remarkable way by Eddington †, using independent material discussed by a new and elegant method. Both results showed that, at least for extensive parts of space, there are a nearly equal number of stars moving in exactly opposite directions. The assumption, then, that the mean of the peculiar motions is zero may, at least for these parts of space, be still regarded as a good working hypothesis.

Adopting an approximate position of the apex of the solar motion, Kapteyn resolved the observed proper motions of the Bradley stars into two components, viz., one in the plane of the great circle passing through the star and the apex, the other at right angles to that plane ‡. The former component obviously includes the whole of the parallactic motion; the latter is independent of it, and is due entirely to the real motions of the stars themselves. From the former the mean parallactic motion of the group is derived, and from the combination of the two components the relation of velocity of the Sun's motion to that of the mean velocity of the stars of the group.

As the distance of any group of stars found by the parallactic motion is expressed as a unit in terms of the Sun's yearly motion through space, the velocity of this motion is one of the fundamental quantities to be determined. If the mean parallax of any sufficiently extensive group or class of stars was known, we should have at once means for a direct determination of the velocity of the Sun's motion in space; or if, on the other hand, we can by independent methods determine the Sun's velocity, then the mean parallax of any group of stars can be determined.

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* Rep. Brit. Assoc. 1905, p. 257.

† Monthly Notices R.A.S. vol. lxvii. p. 34.

‡ Publications Astron. Laboratory Groningen, Nos. 7 and 9.

The Velocity of the Sun's Motion in Space.

If by this method the velocities in the line of sight of a sufficient number of stars situated near the apex and antapex of the solar motion could be determined, so that in the mean it could be assumed that their peculiar motions would disappear, we have at once a direct determination of the required velocity of the Sun's motion.

The material for this determination is gradually accumulating, and indeed much of it, already accumulated, is not yet published. But even with the comparatively scant material available, it now seems almost certain that the true value of the Sun's velocity lies between 18 and 20 kilometres per second*; or, if we adopt the mean value, 19 kilometres per second, this would correspond almost exactly with a yearly motion of the Sun through space equal to four times the distance of the Sun from the Earth.

Thus the Sun's yearly motion being four times the Sun's distance, the parallactic motion of stars in which this motion is unforeshortened must be four times their parallax. How this number varies with the amount of foreshortening is of course readily calculated. The point is that from the mean parallactic motion of a group of stars we are now enabled to derive at once its mean parallax.

This research has been carried out by Kapteyn for stars of different magnitudes. It leads to the result that the parallax of stars differing *five* magnitudes does *not* differ in the proportion of one to ten, as would follow from the supposition of equal luminosity of stars throughout the universe, but only in the proportion of about one to five†.

The same method cannot be applied to groups of stars of different proper motions, and it is only by a somewhat indirect proof, and by calling in the aid of such reliable results of direct parallax determination as we possess, that the variation of parallax with proper motion could be satisfactorily dealt with.

As a final result Kapteyn derived an empirical formula giving the average parallax for stars of different spectral types and of any given magnitude and proper motion. This formula was published at Groningen in 1901‡. Within the past few months the results of researches on stellar parallax, made under the direction of Dr. Elkin, at the Astronomical Observatory of Yale University, during the past thirteen years *, have been published, and they afford a most crucial and entirely independent check on the soundness of Kapteyn's conclusions.

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* Kapteyn, *Ast. Nach.* No. 3487, p. 108; and Campbell, *Astrophys. Journ.* xiii. p. 80.

† *Ast. Nach.* No. 3487, Table III.; and *Ast. Journ.* p. 566.

‡ Publications Astron. Laboratory Groningen, No. 8, p. 24.

In considering the comparison between the more or less theoretical results of Kapteyn and the practical determinations of Yale, we have to remember that Kapteyn's tables refer only to the means of groups of a large number of stars having on the average a specified magnitude and proper motion, whilst the latter are direct determinations affected by the accidental errors of the separate determinations and by such uncertainty as attaches to the unknown parallaxes of the comparison stars—parallaxes which we have supplied from Kapteyn's general tables.

The Yale results consist of the determination of the parallax of 173 stars, of which only ten had been previously known to Kapteyn, and had been utilized by him. Dividing these results into groups, we get the following comparison :—

Comparison Groups arranged in order of Proper Motion.

No. of Stars.	Proper Motion.	Magnitude.	Parallax.		Yale—Kapteyn.
			Yale.	Kapteyn.	
	″		″	″	″
21	0·14	3·8	0·028	0·026	+0·002
39	0·49	6·3	·042	·055	− ·013
45	0·59	6·7	·068	·060	+ ·008
46	0·77	6·5	·047	·074	− ·027
22	1·50	6·2	·118	·124	− ·006

Groups arranged in order of Magnitude.

10	0·61	0·8	0·103	0·110	−0·007
29	·53	3·8	·076	·075	+ ·001
33	·63	5·6	·064	·070	− ·006
34	·73	6·7	·055	·070	− ·017
31	·68	7·6	·025	·061	− ·036
36	·80	8·3	·056	·062	− ·006

These results agree in a surprisingly satisfactory way, having regard to the comparatively small number of stars in each group and the great range of parallax which we know to exist amongst individual stars having the same magnitude and proper motion. In the mean perhaps the tabular parallaxes are in a minute degree too large, but we have unquestionable proof from this comparison that our knowledge of stellar distances now rests on a solid foundation.

[To be continued.]

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* Trans. Astron. Observatory of Yale Univ. vol. ii. part 1.

MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

SION COLLEGE, VICTORIA EMBANKMENT.

Wednesday, 1907 July 3.

F. W. LEVANDER, *President*, in the Chair.

Secretaries: J. G. PETRIE.
H. P. HOLLIS (*vice* J. A. HARDCASTLE).

MR. PETRIE read the Minutes of the previous Meeting, which were confirmed.

The list of presents received was read, and the thanks of the Association were accorded to the respective donors.

The names of four Candidates for Membership were read and passed for suspension, and the election by the Council of three new Members was unanimously confirmed.

Mr. J. McCarthy proposed the election of Messrs. Hugh James and William Schooling as Auditors of the Accounts for the Session 1906–7; this was seconded by *Mr. E. Wm. Johnson* and carried.

The President then read the list of names of Members proposed by the Council to serve on the Council of the Association next Session, stating that Mr. Brooke, whose name appeared on the printed list, wished to withdraw on account of ill-health. The Council had therefore substituted the name of Mr. Krauss Nield. He was glad to say that Mr. Hardcastle was very much better, and he hoped that by the time the new Session commenced that gentleman would be quite able to fulfil his duties as Secretary. It was now open for any Member to propose additional names for any Office or as ordinary Members of the Council. No further names being proposed, the President declared the list closed. He appointed the following gentlemen to act as Scrutineers of the Ballot for the next Council:—Messrs. J. J. Ball, U. H. Stanley, and W. H. Walmsley.

The President said that Sir David Gill had kindly promised to give an address at what should have been the June Meeting of the Association, but unfortunately they could not have the use of the Hall on their usual day, and Sir David was now out of town. He had, however, kindly promised to give an address at the November Meeting. The Librarian wished to give notice that the Library would be closed from July 25th to the end of August.

Mr. Maunder read a paper (illustrated by lantern-slides) on "Observations of Heliacal Settings in April, May, and June, 1907," by *Mr. A. K. Lucke.* He said that Mr. Lucke was a new Member of the Association, and he had undertaken this work at his (Mr. Maunder's) suggestion. Mr. Lucke said that his observatory was the flat roof of his house, in one corner of which was placed a chair always in the same position, close to the parapet of the roof, on which were placed a binocular and sextant, together with his star-charts. He commenced by making a careful sketch to scale of the objects on the western horizon, the scale being 5° = 1 inch, which was quite large enough. His observations included 35 stars, whose magnitudes varied from 0·3 to 5·0, with declinations varying from 35° N. to 18° S. First-magnitude stars were visible nearly down to the horizon on account of the perfect seeing conditions. On one or two occasions he had been able to keep second-magnitude stars in sight till within 2° of the horizon at 1¼ hours after sunset. No telescope was necessary, and stars above mag. 5 disappeared in the binocular about the same time as they did to the naked eye.

The President said that some editors did not seem to have a clear idea as to what heliacal rising and setting really meant, some taking the term to mean the risings of stars when the Sun was rising, and others when the Sun was setting.

The Rev. T. E. R. Phillips read a paper by *Mr. Scriven Bolton* on "North Tropical Change on Jupiter." Between the months of April and August, 1906, a striking phenomenal change occurred in the northern hemisphere of Jupiter. Having passed through a long period of three years of abnormal quiescence, the general surface-features in the N. tropical latitudes exhibited, in the first of the above-mentioned months, signs of renewed activity. The entire tropical regions, including the N. equatorial belt, which became much swollen and intensified, seemed to indicate further changes of development. At this stage, however, Jupiter's conjunction with the Sun necessitated a suspension of observations for three months, during which time the tropical activity in question had evidently been of a decidedly progressive nature. In fact it was seen that the broad and white N. tropical zone had become transformed into a conspicuous dark stripe or band, which constituted the most extensive and unbroken dark tract on Jupiter. It appeared that the entire zone was converted into a dark area in less than eight weeks' time, reaching its maximum intensity at the beginning of August. In the early part of September it was gradually replaced by a lighter tone. This gradual fading out of the shaded area had continued ever since until there was left but a scanty remnant of the marvellously dark and conspicuous stripe which some ten months ago characterized the tropical regions of the disc. The observations of this phenomenal change appeared to indicate that the tropical regions of the planet were relieved temporarily of a superstratum of almost wholly obscuring cloud of a highly reflective nature. Thus they were seemingly able to catch a glimpse into a lower and comparatively dark region of the planet's atmosphere of a reddish and ofttimes a vivid crocus-yellow colour. The observations generally were made with a 10¼-in. With-Browning reflector, power 310. A Steinheil monocentric ocular, magnifying 400 times, was on several occasions used to advantage.

The Rev. T. E. R. Phillips said that the paper was an interesting one, and the observations to which it referred accorded very satisfactorily with those he had been able to make himself during the last apparition of the planet. As a matter of fact, the particular region of the disc to which Mr. Bolton referred—the region of the N. equatorial belt—had been the scene of great and remarkably extensive changes for the last ten years or more, but the change to which Mr. Scriven Bolton had drawn attention was a very conspicuous and remarkable one indeed.

At the request of the President, *Mr. Hollis* gave a *résumé* of a paper by *Mr. C. T. Whitmell* on "Jupiter's Satellites," which consisted of a mathematical explanation of the phenomena described by Mr. Scriven Bolton in a paper read at the previous Meeting of the Association.

Mr. Maunder stated that the fourth comet of the year (1907 *d*) was discovered on June 10 by Prof. Daniel, of Princeton Observatory, New Jersey, U.S.A. It was faint at discovery, but promised to be of greater interest to observers than the first three comets of the year. It would not improbably prove to be a comet of short period, possibly one of the Jupiter family. Towards the end of August and the beginning of September there was reason to hope that it would be very considerably brighter, probably bright enough to be seen by the naked eye.

Mr. Maunder then showed some photographs of the recent very large sun-spot taken by Mr. A. N. Neate. On June 13 it was first seen close to the east limb. The 19th brought the group to the central meridian. The length of the group was 120,000 miles, and its total area about 3,000,000,000 square miles. Mr. Maunder also showed a copy of a photograph taken at Greenwich with the 26-in. photographic refractor.

Mr. C. Thwaites also showed some photographs of the recent sun-spot.

Mr. W. T. Lynn read a paper on "The Condition of the Surface of Mars." He suggested that Herr Fauth's theory that the surface of our Moon was covered with thick ice, beneath which there might be a watery ocean, might be applicable to Mars. The density of the air at the surface of Mars was only a fourth part of what it was at the surface of the Earth. Having so little air to retain the solar heat (of which he received somewhat less than half that which we do) it was probable that his surface was very cold and the greater part of it covered with ice. But there might be water below which might frequently, or perhaps regularly, break through, and the lines along which it did so might occasion the appearances to which the name of canals had been given.

Mr. Petrie then read a paper by *Mr. Samuel Stuart*, of Auckland, New Zealand, on "Ancient Chronological Errors." Mr. Stuart said that, on examining the dates with which ancient chronology presented us, it appeared that such of them as had not been verified by the modern retro-calculation of eclipses were very doubtful, inasmuch as they seemed to have been modified to suit astrological ideas. But all such adaptations had been made long after the events, as was evident from the fact that in every instance those dates had been derived from the tables of Ptolemy's 'Almagest,' written in the second century A.D. So that it appeared that all ancient dates were then recast if not, in many instances, entirely derived from such elements, and were consequently altogether unreliable.

The President said Mr. Stuart evidently devoted a considerable portion of his leisure to such pursuits as was indicated by the present paper—pursuits which he (the President) thought he might say were hardly ever carried out in England.

Mr. Hollis read a short paper by *Mr. Beattie* on "Venus at Mid-day near Inferior Conjunction"; and also an Interim Report of the Variable Star Section, by *Col. Markwick*.

The President said that Col. Markwick was a most energetic Director, and was ably seconded by the Members of the Section. That, he might point out, was Interim Report No. 18, to say nothing of the ordinary Sectional Reports, which were published separately as 'Memoirs.'

The Meeting stood adjourned at 7 P.M.

Relations of the Planets' Orbits to the Invariable Plane and of their Mutual Eccentricities.

LAPLACE, in his theory of Jupiter's four satellites, shows that the orbit of each satellite has a plane peculiar to itself, which plane has a constant inclination to Jupiter's equator, whilst its nodes coincide with the nodes of Jupiter's orbit on Jupiter's equator. To this plane the orbit of each satellite has what Laplace calls its *inclinaison propre*. Marth's figures for these quantities, based on Souillart's theory, are given in M. N. vol. li. p. 513, and have been repeated by Mr. Bryan Cookson in the 'Cape Annals,' vol. vii. p. 66 of part ii. These numbers differ but little from Laplace's, based on Delambre's reduction of old observations, but the recent very valuable work done at the Cape on the Satellites of Jupiter and reduced by Dr. de Sitter and Mr. Cookson shows that Delambre's results are better than Souillart's. Taking Satellite III. as an example, we find its orbit moves on a plane constantly inclined 295″ to Jupiter's equator, and that its inclination to this plane is 644″.

Somewhat similar relations obtain in the system of the eight major planets when their orbital planes are referred to the invariable plane of the solar system.

If ι and Ω represent the inclination and node of a planet's orbit on the invariable plane, the type of the Lagrangian equation is

$$\iota \genfrac{}{}{0pt}{}{\cos}{\sin} \Omega = \Sigma_1^7 \iota_j \genfrac{}{}{0pt}{}{\cos}{\sin} (\beta_j t+\gamma_j).$$

Let the largest term on the right-hand side be $I \genfrac{}{}{0pt}{}{\cos}{\sin} \Pi$, and multiply each side twice, once by cos Π and once by sin Π; we then get

$$\iota \sin(\Omega - \Pi) = \Sigma\iota \sin(\beta t + \gamma - \Pi),$$
$$\iota \cos(\Omega - \Pi) = I + \Sigma\iota \cos(\beta t + \gamma - \Pi).$$

These equations are similar to Marth's referred to above; I is the inclination, and Π the ascending node of the planet's orbital mean plane on the invariable plane; its *inclinaison propre* will be the largest coefficient now left on the right-hand side. Taking Mr. Stockwell's figures as quoted in Prof. Charlier's 'Mechanik des Himmels,' vol. i., but making a correction to the ascending node of Neptune, we have :—

Planet.	Mean Plane of Orbit, 1850.			True Plane of Orbit, 1850.				Orbit on Mean Plane.		
	I.	Π.	ΔΠ.	ι.	☊.	I−ι.	Π−☊.	Proper Inclin.	Proper Node.	Annual Motion of Node.
Mercury	6° 57′	21° 6′	− 5″·13	6° 21′	34° 8′	+0° 36′	−13° 2′	0° 38′	111° 35′	− 1″·46
Venus...	1 18	71 45	−18·41	2 11	53 28	−0 53	+18 17	0 49	309 21	+13·28
Earth ...	1 24	251 45	−18·41	1 35	286 14	−0 11	−34 29	0 36	129 21	+13·28
Mars ...	2 54	292 50	−17·39	1 41	355 10	+1 13	−62 20	2 9	318 55	− 1·02
Jupiter .	0 22	306 19	−25·93	0 20	316 22	+0 2	−10 3	0 4	74 12	+25·27
Saturn..	0 54	126 19	−25·93	0 56	122 49	−0 2	+ 3 30	0 4	254 12	+25·27
Uranus .	1 0	313 56	− 2·92	1 2	310 27	−0 2	+ 3 29	0 4	246 35	+ 2·25
Neptune.	0 40	200 31	− 0·66	0 43	193 0	−0 3	+ 7 31	0 7	291 25	− 2·25

(The figures in columns 2, 4, 9, and 11 are constants.)

It will be noticed that the mean planes of the orbits of Venus and the Earth are nearly equally inclined to the invariable plane, and that their nodes on both planes differ by 180°; again, their *inclinaisons propres* are nearly the same, and necessarily the movements of the nodes are in each case the same.

The *inclinaisons propres* of the orbits of Jupiter, Saturn, and Uranus are nearly the same. The nodes of the orbits of Jupiter and Saturn on both planes differ by 180°. The *noeuds propres* of Uranus and Neptune have equal but opposite motions. The table also shows $I - \iota$ and $\Pi - \Omega$, or the actual deviations of the present orbital planes from their mean positions.

It must always be remembered that the Lagrangian equations are far from possessing mathematical rigour. Dr. Hill's investigation on the secular variations of two co-planer planets tends to show that, in spite of the lack of rigour, the Lagrangian equations afford a fair approximation.

If we turn to the eccentricities and perihelia, there are somewhat similar relations; thus, considering only Jupiter and Saturn, we have

$$e_{\mathrm{IV}}\cos(\pi_{\mathrm{IV}} - 28^\circ\ 9' - 3''\cdot 72t) = 0\cdot 0432 - 0\cdot 0156\cos(99^\circ\ 48' + 18''\cdot 74t),$$

$$e_{\mathrm{V}}\cos(\pi_{\mathrm{V}} - 127^\circ\ 57' - 22''\cdot 46t) = 0\cdot 0484 + 0\cdot 0341\cos(99^\circ\ 48' + 18\cdot 74t).$$

(Similar equations in sines, omitting constant term.)

The mean perihelia and eccentricities are:

Jupiter	28° 9' + 3''·72*t*	and	0·0432
Saturn	127 57 + 22 ·46*t*	and	0·0484

but each planet's eccentricity is acted on by the eccentricity of its neighbour, and in such a manner that it depends on the mean angular distance of their perihelia from each other. Whilst Jupiter's eccentricity is decreasing, Saturn's is increasing, and *vice versa.*

Mr. Stockwell has pointed out that the mean perihelia of Jupiter and Uranus differ by 180°. The relation may be re-written as follows:—

$$e_{\mathrm{VI}}\cos(\pi_{\mathrm{VI}} - 208^\circ\ 9' - 3''\cdot 72t) = 0\cdot 0449 - 0\cdot 0297\cos(283^\circ\ 5' + 0''\cdot 99t).$$

In this case the "proper" eccentricity of Uranus is only 0·0018, and it is overshadowed by the eccentricities impressed on its orbit by Jupiter and Saturn. From Mr. Stockwell's figures it would appear that the proper eccentricity is also smaller than the impressed in the case of the Earth's orbit. In the other cases the "proper" and actual perihelia are as follows:—

	"Proper."	Actual.	Difference.
	° '	° '	° '
Mercury	88 1	75 54	+12 7
Venus	200 50	130 10	+70 40
Mars	317 7	334 1	−16 54
Jupiter	28 9	12 37	+15 32
Saturn	127 57	90 49	+37 8
Neptune	67 57	43 59	+23 58

We also have

	Impressed.	Actual.	Difference.
Uranus	208° 9'	171° 21'	+36° 48'

The eccentricity of the Earth's orbit is nearly equally due to Jupiter and Mars, and in a lesser degree to Venus.

R. T. A. Innes.

The Sun-spots of 1907 *April.*

The decline in sun-spot activity, which began in March, continued during April, and except for the fine procession in the northern hemisphere, formed by Groups 6156, 6158, and 6159, inherited from March, there was very little to chronicle during the month. In the southern hemisphere the chief features were the fourth and last appearance of Group 6081, numbered in this return as Group 6161, and the formation of Group 6168, a new group of long duration. The other spot-groups of the month were quite unimportant, but there was no day upon which the Sun was entirely free from spots.

Rotation 716 began 1907 April $3^{d}.953$.

Spot-Groups in 1907 April.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1907.	1907.	days.	d	°	°	
6161	Apr. 2	Apr. 14	13	Apr. 8·3	303	−18	**Return.** Regular spot.
2	2	2	1	Mar. 30·7	56	+18	Few small spots.
3	3	4	2	31·5	46	−21	Small spot *f.* Gr. 6157.
4	4	14	11	Apr. 11·2	265	−17	Irregular cluster.
5	7	15	9	11·2	265	− 7	Pair of spots *n.* Gr. 6164.
6	10	10	1	6·6	325	+20	Small spot *n.* Gr. 6159.
7	10	11	2	12·1	252	−14	Small spot *f.* Gr. 6164.
8	13	19	7	13·0	241	−11	Large cluster.
9	14	15	2	15·0	214	+ 9	Small stream.
6170	18	25	8	23·9	96	+13	Irregular stream.
1	20	20	1	18·4	169	−13	Very small spot.
2	20	30	11	25·4	76	+ 8	Regular spot and train.
3	21	21	1	21·7	125	−13	Very small spot.
4	22	23	2	26·6	61	+22	Pair of small spots.
5	24	May 2	9	27·0	56	− 6	Irregular stream.
6	25	Apr. 25	1	24·1	94	+15	Small spots *f.* Gr. 6170.
7	25	26	2	25·2	80	−17	Two small spots.
8	25	25	1	28·1	42	− 6	Small spot *f.* Gr. 6175.
9	27	29	3	27·0	56	− 3	Small spot *n.* Gr. 6175.
6180	30	May 4	5	30·8	5	−22	Very small spot.

Notes on the Principal Spot-Groups.

Group 6161.—Return of Group 6140. Fourth and last apparition. A regular spot, area on April 8 about 100, slowly diminishing in size.

Group 6164.—A number of unstable spots in an irregular cluster. The group undergoes many changes. Area on April 7 about 100.

Group 6168.—A number of spots in a large irregular cluster rapidly developing in size. Area on April 15 about 400.

Group 6170.—A number of spots in an irregular straggling and unstable stream. Area on April 21 about 150.

Group 6172.—A regular spot followed by a train of small spots. Area on April 23 about 200.

Group 6175.—A number of small spots in an irregular stream. Area on April 28 about 150.

E. Walter Maunder.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

John Michell.

Gentlemen, —

In the interesting volume of 'Astronomical Essays' recently published by Mr. Gore, it is remarked (p. 112) that "Michell's name is not even mentioned in many biographical works which give a lengthy account of much smaller men, and his name is hardly known to the general reader." There is, however, a very appreciative account of him contributed by the late Miss Clerke to the 'Dictionary of National Biography.' I can hardly assent to the view that "the discovery of binary or revolving double stars is really due to Michell, although usually ascribed to Sir William Herschel." Michell, in an interesting paper communicated to the 'Philosophical Transactions' for 1767 (which was afterwards printed separately), suggested several probable deductions from the proximity of stars in clusters to each other. It was not until 1784 that his second paper appeared, in which he dwelt upon the probability of orbital motion of one component of a double star round another, constituting what we now call a binary star. But this was after W. Herschel had commenced his long series of observations of double stars, and is, indeed, avowedly founded on Herschel's first catalogue, published in the 'Transactions' for 1782. The very great number of these "cannot leave a doubt," he says, "with anyone who is properly aware of the force of these arguments [*i.e.* relating to the probable connexion between stars very near together in the heavens], that by far the greatest part, if not all of them, are systems of stars so near to each other as probably to be liable to be affected sensibly by their mutual gravitation; and it is therefore not unlikely that the periods of the revolutions of some of these about their principals (the smaller ones being, upon this hypothesis, to be considered as satellites to the others) may some time or other be discovered."

Thus far, however, the matter remained one of probable conjecture, and we cannot doubt that the same idea forced itself also upon the attention of Herschel, who first took up the subject of the observation of double stars in the hope of obtaining the differential parallax of the brighter components, but was drawn away, as his son tells us, by the unexpected frequency of the duplicity, to suspect physical connexion, and therefore repeated his observations till it was quite evident that in several cases there was real orbital motion of one star round another, or rather, of course, of both round their common centre of gravity. It is in this shape that we must consider the existence of binary stars as a discovery and an important one, though even Herschel stopped short of actual determination of stellar orbits, a calculation first made by Savary (in 1827) in the case of ξ Ursæ Majoris.

It must be remembered that Michell and Herschel were no strangers to each other. Both were musical as well as astronomical, and Herschel had often played the violin at entertainments given by Michell, who also gave him his first lessons in the art of speculum-grinding. Doubtless, then, they held communications on the new light thrown upon the subject of double stars by Herschel's observations.

It will not, I think, be supposed that I have any desire to belittle the far-reaching sagacity of Michell in his conjectures with regard to stellar motions, but the chief merit of a discovery rests with him who proves. In one, indeed, of Michell's ideas he was clearly wrong; I mean with regard to the supposed effects on the corpuscles of light by the attraction of the luminous bodies emitting it. He held for some years the post of Woodwardian Professor of Geology at Cambridge, and was Rector of Thornhill in Yorkshire from 1767 until his death in 1793, at the age of sixty-nine. He invented the torsion balance for measuring the density of the Earth, but did not live to apply it, which was done by Henry Cavendish, one of the discoverers of the composition of water, though whether the priority rests with him or Watt it seems difficult to determine.

Michell was of Queens' College, Cambridge, and graduated as Fourth Wrangler in the second year of the establishment of the Mathematical Tripos.

Yours faithfully,

Blackheath, 1907, July 6. W. T. LYNN.

D_3 *Absorption in the Solar Spectrum.*

GENTLEMEN,—

Since writing to you last about D_3, I have been paying some attention to what I have been suspecting for some time, that the absorption line of D_3 is not altogether absent from the ordinary solar spectrum. As early as July of last year I had found that the darkening line near spots could be traced right through the solar spectrum as a faint line. I had afterwards made some drawings of this region on fine days. These show a couple of faint lines close together in the ordinary solar spectrum.

The fainter of these two and the one on the more refrangible side has been marked as darkening near spots. I could not believe that a line not after all so very difficult of observation could have been missed all this time. Further, all my observations were made in the mornings with a low Sun and with a prism spectroscope in which the dispersion in this region was not particularly great. Atmospheric lines characteristic of this region might easily mislead one, and I could not avoid them, for as the day advances the definition here gets bad and finer observations are impossible. Again, I had also received an impression from several observations that the absorption-line and the bright D_3 in the chromosphere or in the umbræ of spots did not exactly coincide, but that the latter was always a little to the less refrangible side. In these circumstances I could not feel sure I had observed the absorption-line of D_3 in the ordinary solar spectrum. But very recently the large grating spectrograph of this observatory has been brought into use, and on the 19th inst. I was able to secure a photograph of this region. With a single exposure I got both the darkening and the chromospheric (bright) line when a spot was close to the limb. The negative is rather thin, but it shows distinctly the dark and the bright lines, and both as a continuation of a faint line in the ordinary solar spectrum. I have not yet made any measurements on the plate, nor has the weather been fine these several days to go into the question of the origin of this faint line which seems to coincide with D_3.

Yours faithfully,

G. NAGARAJA.

The Observatory, Kodaikánal,
1907, April 23.

GENTLEMEN,—

As far as my own observations are concerned, since I addressed you on this subject (No. 379) but few new points can be added. The observations of Mr. G. Nagaraja and of Capt. Daunt are practically wholly corroborative of my own and very valuable, as they mutually lend weight to the correctness of the observations reported. I made many observations of D_3 in absorption during the years 1895–1904, and found myself during that period a rather solitary observer, provoked to insistence on looking for this particular phenomenon by the, up to that time, entirely isolated observation by Prof. Young in 1870. My reports were given "the benefit of the doubt," and it seemed that the cases described when the phenomenon was seen by me exceptionally strongly, were considered rather as evidence of its extreme scarcity.

The great spot of 1905 January–February, however, afforded exceptional opportunities for spectroscopic observations, since its evolutions were on such an intense scale. I brought the subject of dark D_3 again before our local Astronomical Society in March 1905, and a few days later Prof. A. Fowler did likewise before the R. A. S., we two being mutually unaware of the special observations we made during the transit of this exceptional group. By its very extent this group gave me the key as to which portion of a disturbance to pay special attention to, and proved an immense help in further investigations, considering the smallness of my instrument and that the focal image is scarcely ½ inch diam. on the slit-plate. The same spot-group also afforded the opportunity of making trials with different powers of eyepieces in the view-telescope, and I have no hesitation in saying that I attribute the ease with which I now pick up dark D_3 to increased eyepiece power, as well as to an acquired knowledge where to look for the likely appearance of the effect. My refractor of 3 inches aperture has a focal length of 1300 mm., and the eyepieces I almost exclusively use have an equivalent focus of 18 and 15 mm., and are of the Zeiss monocentric type. My advise to intending spectroscopic observers of the Sun is to use higher power eyepieces than those supplied as a rule with spectroscopes by the makers, and have them of the very best only.

Contrary to Mr. Nagaraja's surmise, I have not overlooked reversals, and he will find such referred to in my first letter to you on this subject (No. 358). But Mr. Nagaraja calls special attention to "faint" reversals, yet past experience encourages me to assert that, as a rule, the stronger the reversal the darker D_3. Now strong and brilliant reversals of Hα are, in the majority of cases, accompanied by deflections of the line, and when Hα is brilliantly reversed and strongly deflected, an irregular and deflected *dark* Hα near by may also be looked for with almost certainty. There is, indeed, in my records quickly accumulating evidence that these adjoining dark deflections, notches, &c. are the direct continuations of the selfsame prominence. In the *English Mechanic* of June 7, 1907, I described what I have seen repeatedly, viz., a brilliant uprush with reversal and deflection of line, connected with deflection of the same line in absorption, the latter ascending to a sufficiently great height to project over the limb, where it became visible as a not very bright prominence *. I was assured by Prof. Hale, on the occasion of his recent lecture at Burlington House, that he has spectroheliographed the same phenomenon, and Prof. Mitchie-Smith also told his audience that he has caught such a case in calcium light. Incidentally I wish to remark on the puzzling fact that the bright reversal and its dark continuation are in most cases almost sharply divided, as if the transition from the bright reversal to its dusky absorption took place rapidly at a definite level, causing the most brilliant reversal to be in apparent direct contact with intensely dark agitated volumina of the same element. Contrast effects cannot be the cause of this sharp division, nor would revolving of the masses in the line of sight explain it, because in that case the transition would be more gradual.

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* (*July* 1907) Other instances were seen on the 12th and 18th inst.—A. A. B.

To ordinary telescopic vision it is not disclosed whether "faculic" areas are purely faculic or whether prominences are also contained within the same area. The spectroscope as a rule reveals these, even if they are not violently eruptive, thus indicating their presence on the disk by the faint reversals alluded to by Mr. Nagaraja. If so, I should feel inclined to attribute their presence to a higher stage of activity than if the area was purely faculic. This would explain why over some faculic areas dark D_3 is seen plainly and over others not, and I believe the former cases to be confined to the activity zones, From this it would follow also that the appearance of dark D_3 is an index of enhanced activity, and the magnificent spot-group of the past two weeks showed this prominently. The most conspicuous absorption effects in D_3 were locally identical, as well as synchronous with the recurring ups and downs of special activity, beautifully shown by the brilliant prominences seen in projection.

There are the many instances in which D_3 is seen without any spots being formed within the area under observation; indeed their formation to a certain extent seems to deprive us of seeing dark D_3, most probably because in spots the bright background afforded by the photosphere or chromosphere is removed or obscured. All our observations show that we cannot follow dark D_3 across spot-umbræ, but I feel hesitation to assert that this means that helium must be absent. On the other hand it certainly shows no widening effects, as do, for instance, the sodium lines, but the dark line seems to run into fine points on either side, as it approaches the umbra.

As regards seeing D_3 bright on the disk, I have some eight or nine instances in my records, and I freely say that its observation is as difficult as it seems rare. Only the superlatively violent disk-eruptions afford the spectacle, such as those of 1907 March 3, when it appeared all about the visible starting-point of the most gigantic prominence I have ever seen, and which, as regards both length of path traversed and velocity attained, constitutes a record, leaving far behind those grand outbursts recorded by Prof. Hale and Prof. Fényi. Other fine exhibitions of bright D_3 on the disk were seen by me on February 2, May 13, July 8 and 14, and November 15, 1905, April 14, May 13, June 11, 1906.

It is certainly remarkable how readily dark D_3 makes its appearance directly over the most brilliant hydrogen reversals in disk eruptions, yet I could not speak of a single instance of having seen D_3 in absorption in the upper portions of the most brilliant prominence on the limb or over any other portion of them. Still, seeing often the extraordinary dark and apparently dense hydrogen accumulations projected on prominences on the limb, I think that dark D_3 ought also be visible, and feel sure that a better instrument than mine will ultimately bring it out. Reverting to its appearance on the disk, I think that, apart from any considerations of temperature, highly elevated volumina of hydrogen or helium (no doubt also calcium) have a much reduced depth of coronal absorption superposed, and their genuine emission and absorption effects have a chance of meeting our vision unimpaired.

I am, Gentlemen,
Yours faithfully,
ALBERT ALFRED BUSS.

2 Lansdowne Terr., Grosvenor Sq.,
Ashton-on-Mersey, near Manchester,
1907, June 29.

P.S. — Naturally I am greatly interested in Mr. Nagaraja's investigations and look forward with expectation to the result of his examination of the negatives obtained and his measurements, which he mentions in the letter you have given me the opportunity of seeing. I have previously in these columns expressed my conviction (*Observatory*, No. 379, January 1907, p. 63), on theoretical grounds, that improved instrumental means and diligent special search might bring out D_3 as a fine dark line in the general solar spectrum in permanency. I have also (*Observatory*, No. 360, August 1905, p. 319) called attention to the difficulty of sorting D_3 out from its immediate surroundings, and shown the difference with which D_3 appears to direct vision in the spectroscope as compared with the atmospheric lines near by. I should certainly feel pleased to hear that Mr. Nagaraja has succeeded in establishing absolute agreement between the true D_3 and the fine dark line which he suspects of representing helium in the Fraunhofer spectrum.—A. A. B., *July* 9.

The Eclipse of Thales.

GENTLEMEN,—

I lately met with a passage in the 'Stromata' of Clement of Alexandria which bears on the eclipse of Thales. Clement himself I place no reliance on, but it may be otherwise with Eudemus, who appears to be his authority. (I have only an English translation of the 'Stromata,' and suspect that the translator has supplied the word "son," as is often done in similar cases.)

"Eudemus in the 'Astrological Histories' says that Thales foretold the eclipse of the Sun which took place at the time that the Medians and Lydians fought in the reign of Cyaxares, the father of Astyages, over the Medes, and of Alyattus, the son of Crœsus, over the Lydians. Herodotus, in his first book, agrees with him. The date is about the fiftieth Olympiad." * As the Christian era is referred to the 195th Olympiad, this would give about B.C. 580 for the date of the eclipse, but whether the other historic *data* agree with this I leave to others to decide. It seems at all events to accord much better with the eclipse of B.C. 585 than with that of B.C. 610. Clement's object is to show that the Jewish philosophy is much earlier than the Greek, but he might have placed this eclipse 30 years earlier without much injury to his argument.

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* Book I. cap. 15.

Truly yours,

Dublin, 1907, June 3. W. H. S. Monck.

Illumination of Telescope Fields.

Gentlemen,—

Allow me to correct a mistake into which I fell, from lapse of memory, at the last Meeting of the R. A. S., as reported in the July number of the *Observatory.* The method of central illumination of the field of the Cape Transit-Circle was introduced in 1895 or 1896, not 28 years ago, as I inadvertently mentioned at the Meeting. The method is described in the Introduction to the Cape Meridian Observations, 1896–97.

Central electric illumination has been, however, used in all other instruments at the Cape from about 1887.

Horton Court, Chipping Sodbury,
1907, July 9.

Yours faithfully,
David Gill.

OBSERVATORIES.

Oxford University.—The staple work of this Observatory continues to be the printing of the Astrographic Catalogue, an arduous piece of work which entails preparation of copy for press, and some re-measurement of the less satisfactory plates, besides proof-reading, and is likely to occupy the staff for some years. Volumes I. and II. (Zones +31° and +30°) are printed and distributed, and it is stated in Prof. Turner's Report, dated April 30 last, that Volume III. is printed. The whole Oxford Section will occupy eight volumes.

The proposal for a house for the Director at the Observatory has been again brought forward and rejected, or rather, as the opposition took the form of raising other questions for prior consideration, the main question may be considered to be merely deferred. One of the questions referred to contained the suggestion of a possible fusion with the Radcliffe Observatory in the future, a course which would not seem to be to the advantage of astronomy, as it would entail the loss of one directing head.

Cambridge. — The following information is taken from Sir Robert Ball's Report to the Observatory Syndicate of work done in the year ending 1907 May 18.

Meridian Circle.—The work with this instrument, which is in the charge of Mr. Hartley, consists of the observation of zodiacal stars in Sir David Gill's Catalogue. The observations made with this instrument in the years 1872–1900 are being printed in ledger form.

Sheepshanks Equatorial.—This instrument is used for taking photographs for determination of stellar parallax. During the year 185 plates have been taken for this purpose. Since the work was begun in 1904, 30 fields have been removed from the working-list as completely observed, 61 fields are under observation, and 50 are waiting to be begun as opportunities offer.

This work is in charge of Mr. Hinks, who also has in hand the determination of solar parallax from a general combination and solution of all the observations of the planet Eros made at the opposition of 1900–1901. This work has made good progress, and the measurement of the Cambridge photographs of Eros is proceeding rapidly.

Floating Zenith Telescope.—This instrument, the property of Mr. Bryan Cookson, is being used by him for a determination of the constant of aberration. This is a photographic telescope, and the investigation requires that on every favourable night a group of stars should be photographed in the evening and another in the morning. As in the case of the stellar parallax work above mentioned, it is found extremely difficult, owing to the climate, to carry out a scheme of this kind quite satisfactorily. Photographs are also being accumulated for a determination of the variation of latitude.

Mr. F. J. M. Stratton, Isaac Newton Student, who is an Honorary Member of the Staff, is making an investigation of the proper motion of faint stars in the Pleiades, by comparison of early plates taken at Greenwich and Oxford with recent plates taken with the Sheepshanks telescope. He is also engaged upon a new reduction of Schlüter's heliometer observations of the Moon made in 1841–43, the necessity for which was pointed out by Mr. Saunder in this Magazine (1906 Nov., p. 434).

The report of work done with the Newall Telescope is signed by the Assistant Director, Mr. H. F. Newall. This instrument was used partly in making some preliminary solar observations on the possibility of the atmosphere being good enough for refined work, and as the result showed that good conditions for observing were available between 6 and 9 A.M., a permanent horizontal equipment for solar work is in course of construction, the expense of which is to be met by an appropriation from the McClean bequest.

The 25-inch object-glass was remounted on the equatorial in the autumn of 1906, and since 1907 January plates of the red end of the spectra of some of the brighter stars have been taken, firstly with a four-prism spectrograph, and more recently with the diffraction-grating spectrograph.

Mr. Cookson has been appointed to the new post of Assistant in Astrophysics, created in 1906 December. Besides his work before mentioned, he is taking part in the investigations of stellar spectra and in a new research in spectral photometry.

Mr. Hubrecht, Research Student in Astrophysics, continues his work on the rotation of the Sun by a spectroscopic method.

Royal Observatory, Edinburgh.—Prof. Dyson's report for the year ending 1907 March 31, the Seventeenth Annual Report of the Astronomer Royal for Scotland, is printed as a parliamentary paper.

With the meridian instrument the programme consisted of observation of zodiacal and heliometer comparison-stars of Sir David Gill's lists, and, during the year, 2422 transits and 2283 zenith-distances were observed on 64 nights. Some change has been made in the system for relieving the weight of the instrument, which has reduced the horizontal flexure to 0''·5, which is half its former amount. The method of illuminating the field has been altered; a recording drum has been applied to the zenith-distance micrometer, and a registering micrometer for transit observations has been added.

A few observations were made with the 18-inch equatorial, and 50 photographs of an experimental character were taken with the 24-inch reflector. It is hoped that the photographing of the positions of small nebulæ is work which can be usefully undertaken with this instrument.

Spectroscopic observations to determine the velocity of the Sun's rotation were made on 76 days.

The Milne Seismograph has been in continuous operation. The San Francisco earthquake of 1906 April 18, and that at Valparaiso on August 17, both produced sensible oscillations on the records, as did another shock in the Aleutian Islands, which overlapped the latter. The Kingston earthquake of 1907 January 14 was recorded, but the amplitude was snall.

The daily time-service of the cities of Edinburgh and Dundee is supplied by this Observatory. There is a considerable amount of meteorological information in the report, and among this is given the fact that on September 1 and 2 the maximum shade-temperature was 85°·3, the highest as yet recorded at the Royal Observatory since its establishment on Blackford Hill.

PUBLICATIONS.

The Moon in Modern Astronomy *.—Many who know the name of Herr Fauth as that of a keen and assiduous observer of the Moon will take up this book with expectations of which but a small proportion will be realized; the prevailing sense on laying

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* By Philip Fauth, translated by Joseph McCabe; with an Introduction by J. E. Gore, F.R.A.S., M.R.I.A., &c. London: A. Owen & Co., 28 Regent Street. Pp. 160. Price 10s.

it down will almost certainly be one of disappointment. The avowed object of the book is to present in a popular form an account of the development of selenography; but Herr Fauth holds very peculiar views as to the present condition and the past history of our satellite, and his presentation of the subject, and, we fear also, his want of appreciation of the work of others, would give the uninitiated reader a very incorrect idea of what is now generally held. He commences with a regret that the only selenographical works recently issued in Germany have been his own and the first volume of Krieger's Atlas, and yet it would seem that Dr. Franz's endeavours to lay the foundations of the science on the basis of accurate measurement fall within this category and deserve more recognition than they receive from Herr Fauth. They are not mentioned in the Historical Survey, where Franz's work is dismissed in two lines, and his contour-map of the surface, which is reproduced on p. 56, is attributed to Dr. Mainka. Herr Fauth gives full credit to Schröter, Lohrmann, Mädler, and Schmidt for their work in mapping the Moon, but does not seem to realize that the measures on which this work was based are not such as to satisfy modern requirements.

In Chapter II., which is headed "Appearance and Reality," Herr Fauth calls attention to the fact that the lunar mountains appear much steeper than they really are, and gives measures of his own which are in fair agreement with those obtained by Schmidt, though on the whole they give slightly smaller values to the slopes.

In Chapter III., on "Light and Colour," he tells us that the surface of the Moon gradually bleaches under the action of sunlight, so that the maximum of brightness occurs after Full Moon, and the third quarter is brighter than the first. This is certainly true of some parts of the surface, but others darken under the action of the Sun's rays, and the comparison of the illuminations received from the Moon at first and third quarter is complicated by the fact that they are received from entirely different parts of the surface. Photometric results would be very interesting, but none are given.

Herr Fauth believes that the bright ray across the Mare Serenitatis which passes through Bessel does not belong to the Tycho system at all, and suggests that it may proceed from a radiant on the other side of the Moon; but having expressed his own disbelief in the existence of rays of anything like 3000 miles long, he proceeds to say on the next page that "Experts speak of rays 3000 miles long," and uses this as an argument against their having been caused by a meteoric splash.

In Chapter IV., on "Ring Mountains," Herr Fauth rejects all terrestrial analogies, and asserts that even the small crater-cones to which Neison and Klein have called attention will not bear a direct comparison with terrestrial cones. No reference is made to the Hawaiian field, the resemblance of which to many of the lunar formations has been recently confirmed by Prof. W. H. Pickering. There are frequent instances on the Moon of a pair of overlapping craters one only of which is complete; a typical example is found in Theophilus and Cyrillus. Contrary to the received opinion, Herr Fauth holds that the incomplete crater is the more recent, having grown on to the other.

In Chapter V. "The remaining Elevations and the Rills" are considered, and Herr Fauth gives a qualified endorsement to the theory that meteoric impact gave the first impulse for some at all events of the formations. In several places he has spoken of selenographers as having been hypnotized by Laplace's nebular hypothesis and the plutonic theories advanced by such geologists as Suess or such selenographers as Loewy and Puiseux. He now makes it plain that his objections are not to mere details, and claims that "selenography has discovered abundant evidence to refute the old idea that the Moon is an offshoot of the Earth," so that the history of the Moon's genesis, proved almost to demonstration by Sir George Darwin, is completely thrown over.

In Chapter VI. he states his own "Conclusions," which would seem to be that the Moon has a "metallic earthy nucleus" of specific gravity about 4·5, and that this is surrounded by an ocean of water some 115 miles deep, the outer surface of which at a temperature of −273° C. is completely frozen. The inaccuracies of statement by which this amazing theory is supported are too numerous to particularize, but it may be noted that amongst them Herr Fauth quotes Prof. Langley's conclusions from his Mount Whitney experiments, although these were explicitly withdrawn by their author in favour of his direct measurements with the bolometer. Very is quoted as stating that the ground at the sub-solar point "increases its temperature by more than 100° C. (which would be −173° C.)," whereas Very really speaks of a temperature of +181° C., stating that "only the most terrible of Earth's deserts, where the burning sands blister the skin, and men, beasts, and birds drop dead, can approach a noontide on the cloudless surface of our satellite." The complete absence of atmosphere is stated to be confirmed by observations of the "eclipsing" of Jupiter and Saturn, no allusion being made to Prof. W. H. Pickering's claim that visual and photographic observations of occultations of Jupiter have afforded evidence of a rare atmosphere on the illuminated limb. Herr Fauth apparently does not consider the possibility of evaporation from his coat of ice.

He attributes the systems of bright rays to clouds of ice-powder pressed out by their own weight through holes and passes in the ice-walls of the rings from which they radiate; how much spreading might be expected under such conditions is not considered.

One of "the remarkable consequences of this theory" is that "those astronomers are right who place the centre of gravity of our satellite beyond the absolute centre of the sphere," though

Mr. Cowell cannot detect the Hansen effect in his analysis of the Greenwich observations.

Herr Fauth rejects entirely all supposed changes on the Moon: this he has a perfect right to do, the decision resting on a balance of evidence; but he has no right (so far as the present writer is aware) to suggest that Schmidt did not support the supposed changes in Parry B and Alpetragius *d* because "he must have felt himself that he had gone too far" in the case of Linné. This amounts practically to a charge of bad faith, and no evidence is offered in support of it.

That Herr Fauth has been a diligent observer is shown by his claim to have added 4590 new craters and 1258 new rills to the map of the Moon. The most valuable parts of his book are the reproductions of some of his own charts, and the most interesting those in which he refers to his own observations. It is in every way to be regretted that one who has such stores of good grain from which to draw should have given us so much that fails to satisfy.

Mr. Gore, in his Introduction, gives a few well-known facts with regard to the Moon, but makes the statement that "the Sun never sets on the Leibnitz and Doerfel Mountains." A simple calculation shows that a mountain 30,000 feet above the level of its horizon must be within 6° of the terminator in order that it may just catch the rays of the Sun. As the pole may itself be 1°·5 from the terminator, the lowest latitude permissible in order that the peak of such a mountain may have continuous sunlight is about 85°·5. The Doerfels terminate some 10° from the pole and are therefore altogether beyond the possible limit. The Leibnitz Mountains have high peaks near the pole, and the statement may be true for one or two of these, but as originally made it lends itself to misconception.

German is not an easy language to translate into idiomatic English, and the translator has not always been successful in producing even an intelligible sentence, whilst no one who had any acquaintance with lunar topography would have committed himself to the statement that Copernicus is "scantily encircled with walls," or have spoken of "Pico and Piton on Plato" and the "peak Gamma on Lambert." The writer does not feel quite sure whether Herr Fauth or his translator is to be held responsible for the statement that the reappearance of a star after occultation would be retarded by a lunar atmosphere.

SOME LONGITUDE DETERMINATIONS.—During the past few months accounts of three measurements of arcs of longitude of different character have kindly been sent us, and though we can scarcely spare space to describe each in full detail, there are points in each which it may be of interest to mention. The first comes from Dr. Bakhuyzen, at Leiden, and is the account of a measurement made in the year 1893 of the arc between Leiden and Ubagsberg, which is not of much interest by reason of its situation, for it is merely a link in the continental geodetic network, but the point one feels inclined to call attention to is an instrumental peculiarity often found in such work. The telescopes used for time-determination were two transit instruments of the *coudé* type, and the collimation correction was deduced from observations of close circumpolar stars in the two positions of the instrument, eyepiece east and eyepiece west. The consistency of the results gives considerable reason for thinking that the adopted collimation-errors, which are the means of several determinations made during an evening, are correct; but when these are applied the clock-error determined with the telescope in position eyepiece east is different to what it is when the instrument is in position eyepiece west by, in the case of one of the instruments, nearly $0^{s}\cdot1$ in the mean. For the other instrument the corresponding difference is $0^{s}\cdot02$. This is merely quoted as an example of the instrumental idiosyncrasy mentioned at the Meeting of the R. A. S. reported in our last number; but, as was then explained, under certain circumstances this does not affect the final result. The probable error of the determination comes out $\pm0^{s}\cdot015$.

The second is a determination of the difference between the meridians of Potsdam and Brocken made by officers of the Prussian Geodetic Institute in 1906 by their well-known method of observing the transit of every star with the instrument in both positions, whereby such discordances of collimation as those mentioned above are not exhibited. The probable error of this determination is given as $\pm0^{s}\cdot009$. But the main interest of this work is that, at the same time as the longitude was being done, signals were sent by wireless telegraphy from a place 32 kilometres from Potsdam and 183 from Brocken, and received on the chronographs at the two longitude stations. The differences of the clocks thus found were almost identical with the differences found by the ordinary method of telegraphy, so that it may be assumed that longitude by wireless telegraphy is a practical possibility. The duration of transmission was negligible, but it is said that atmospheric influences were more effective than in the case of ordinary telegraphy.

The third account we refer to is a deternination through the all-British Pacific cable, made by Dr. Otto Klotz in 1903, the result of which was given in the number of this Magazine for July 1906. It appears from the details now published that the two observers, Dr. Klotz and Mr. Werry, measured separately each link of the chain formed by the cable-stations and added the results. That is, Dr. Klotz being at Vancouver, and Mr. Werry at Fanning, they measured the arc which separated them. Dr. Klotz then went to Suva, Fiji, and Mr. Werry remaining at Fanning the work proceeded. It will be noted that the result Vancouver to Suva is therefore independent of personal equation. When this arc was completed, Mr. Werry travelled to Norfolk Island, and the third arc was measured, and the differences of longitude between

Norfolk Island and Brisbane, and Norfolk Island and Wellington, New Zealand, were subsequently determined.

We thus have determinations of the differences of longitude between Greenwich and the Australian colonies by a new route with an accuracy measured by a probable error of $\pm 0^{s}{\cdot}06$ or $\pm 0^{s}{\cdot}07$, which includes the probable error of the determination of the initial longitude of Vancouver, and so is not a gauge of the accuracy of the present work. The probable error attached to the results of the individual arcs range from $\pm 0^{s}{\cdot}007$ to $\pm 0^{s}{\cdot}021$, excluding the short arc from Doubtless Bay to Wellington, New Zealand, which has a probable error of $\pm 0^{s}{\cdot}045$. The book containing these details comprises also a valuable bibliography of the subject of antipodean longitudes and comparison of the present results with other values of the longitude of the same places measured eastward from Greenwich, which shows that the longitude round the globe has been measured to be $360^{\circ}\ 0'\ 1''{\cdot}02$, the discrepancy being equivalent to 84 feet. When Capt. Fanning, commanding a trading-vessel, in 1798 found the island which bears his name, he said "these islands are in longitude $159^{\circ}\ 12'\ 30''$ W." The Admiralty gives $159^{\circ}\ 23'\ 27''$, and the present telegraphic determination gives for the longitude of the spot referred to on the Chart as $159^{\circ}\ 23'\ 26''{\cdot}61$, so that there is practically perfect agreement with the Hydrographic Office, and Capt. Fanning was not so very far wrong, remembering his resources.

NOTES.

Comet Notes.—Later observations have considerably modified the elements of Comet *d* 1907 (Daniel). The following determination is by Herr Kritzinger, from observations on June 15, 24, and July 4:—

$$\begin{array}{ll} T \ldots\ldots\ldots & 1907 \text{ Sept. } 4{\cdot}168 \text{ Berlin M.T.} \\ \omega \ldots\ldots\ldots & \left.\begin{array}{r} 293^{\circ}\ 49'{\cdot}16 \\ 143\ \ \ 2\ {\cdot}45 \\ 8\ \ 56\ {\cdot}37 \end{array}\right\} 1907{\cdot}0. \\ \Omega \ldots\ldots\ldots & \\ i \ldots\ldots\ldots & \\ \log q \ldots\ldots & 9{\cdot}71590 \end{array}$$

The following ephemeris was computed by Dr. Smart from earlier but very similar elements; it has been approximately corrected to agree with the above elements:—

	R.A.			N. Dec.	
	h	m	s	°	′
Aug. 3....	4	50	27	16	39
5....	5	9	33	17	1
7....	5	28	12	17	16
9....	5	46	41	17	25
11....	6	5	24	17	26
13....	6	23	16	17	25
15....	6	40	44	17	16
Aug. 17....	6	57	44	17	5
19....	7	14	12	16	54
21....	7	29	58	16	38
23....	7	45	11	16	14
25....	7	59	48	15	42
27....	8	13	58	15	12
29....	8	27	38	14	42

The comet is nearest to the Earth on August 1 (distance 0·75); it will be brightest on August 21 (about 19 times that at discovery).

On August 5 it will rise 3¾ hours before the Sun.
" 21 " 2¾ " "

A recent Greenwich photograph shows 4 tails; there is every likelihood that the comet will be faintly visible to the naked eye during the first half of August. A. C. D. C.

Minor Planet Notes.—The three planets whose periods are nearly the same as that of Jupiter have been named after heroes of the Siege of Troy, 588 TG being Achilles, VY Patroclus, XM Hector. It seems strange that Achilles and Patroclus should be placed on opposite sides of Jupiter. A. C. D. C.

Obituary.—**Charles Trépied**, whose death was briefly announced in our last number, died on June 10 in the 63rd year of his age, leaving a widow but no children.

Between the years 1870 and 1880 there was a considerable revival of astronomical activity in France, which had its outcome in the establishment or re-organization of Provincial or Colonial Observatories—Bordeaux, Besançon, Toulouse, Marseilles, and others. In 1880 the Committee which had these things in their charge decided to establish an Observatory at Algiers, and chose as its Director M. Charles Trépied, who was already known as a writer on astronomical subjects. There was already an observatory, but in a languishing condition, at Ouargla, and it was of this minor institution that M. Trépied first had charge until the larger and more complete Observatory was established at Boudzaréah under his direction in 1885. Evidence of M. Trépied's energy is found in the amount and variety of work that he did with the help of only one assistant in the earlier and less well-equipped building—meridian work, meteorology, chronometer work for vessels entering the port, and computations for the 'Connaisance des Temps,' to which he added solar photography and spectroscopy.

In the inception of the Astrographic Chart and Catalogue M. Trépied took an active part, and at the first Congress in 1887 April, he, with M. Dunér, was chosen as Secretary. A zone of the work was allotted to the Algiers Observatory, and several sections of the catalogue of this zone have been published, and a large number of reproductions of its chart-plates distributed. M. Trépied also contributed memoirs to the Bulletins on technical points connected with the work. M. Trépied's geniality and friendliness, which was especially evident on the occasion of the solar eclipse visible at Algiers in 1900, made him very popular with those who had the pleasure of his acquaintance, and his loss will be deplored by many.

E. von Oppolzer. We also hear with regret of the death of Prof. Egon von Oppolzer, son of the celebrated Theodor von Oppolzer, at the early age of 37. He was born at Vienna in 1869, and was educated at the Universities of Vienna and Munich. In 1897 he became an assistant in the Observatory at Prague, where he discovered, in 1901, the variability in the brightness of the planet Eros. In the latter year he was appointed Extraordinary Professor of Astronomy at Innsbruck, where he remained until his death. Among the subjects on which he wrote are astronomical refraction, solar physics, and the application of physical theory to stellar problems. He also made contributions to meteorology. A new form of zenith-telescope was constructed by him, as well as a photometer of novel design. The variability of the minor planets, which has recently become a subject of very great interest, has naturally been investigated with the greatest success by the aid of photography, and it is worthy of note that Dr. von Oppolzer's important discovery in this branch of research was established by visual observation *.

Dr. H. Kreutz. We also have to record with regret the death of Dr. Heinrich Kreutz, which happened on July 13 last, in the 53rd year of his age. Dr. Kreutz was Editor of the *Astronomische Nachrichten*, which office he succeeded to in 1896 on the death of Prof. Krueger, his father-in-law, who had been in charge of that publication since the year 1880.

Dr. Carl Braun, who died on June 3 last (see July number), was a man of untiring industry, and showed his capability in many branches of astronomy. He established the Observatory of Kalocsa, in Hungary, and inaugurated the solar physical work of that Observatory, which is so abundant. He wrote on such subjects as cosmogony and the density of the Earth. He made an early suggestion for a self-registering transit-micrometer, and he is also credited with a plan for photographing the Sun by monochromatic light, as is now being done in various places by the spectroheliograph.

The Astrographic Chart.—We are informed that the Observatory at Potsdam does not propose to take and distribute the long-exposure photographs (Chart Plates) of the Zone (Dec. +32° to +39°) that has been allotted to them, but that this work will be done by the Royal Observatory of Belgium, which is being re-organized on a somewhat liberal scale.

The list of recent visitors to Greenwich includes the names of Dr. Backlund (of Poulkova) and Prof. Kostinsky; Dr. Thome, of Cordoba, with Mrs. Thome and family; Dr. Elkin, of Yale College

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* *Nature*, July 25.

Observatory; M. Georges Lecointe, the lately appointed Director of the Royal Observatory of Belgium; Mr. Wheeler Wilson, Professor of Astronomy in Harvard University; and Prof. Charlier, Director of the Observatory at Lund, Sweden.

In a short note in *Ast.-Nach.* 4182, Mr. Frank Schlesinger, of the Allegheny Observatory, suggests that in refined investigations of latitude some account should be taken of the fact that the liquid from which the observer determines his level is subject to tidal attraction of the Sun and Moon, and that the effect of this is perceptible. He says that in the extreme case it is possible for the mean of five nights' observations of a latitude group to be in error by $0''\cdot015$.

In No. 4176 of the *Ast. Nach.* Dr. Auwers has compiled a list of abbreviations for the names of star-catalogues in general use from Baily's Flamsteed, which he calls B. Fl. instead of B. F., as is the Greenwich custom, to the Greenwich Second Nine-Year Catalogue, designated $9\ y_2$.

The Royal Society of Edinburgh has awarded the Makdougall-Brisbane Prize for 1904–6, consisting of a gold medal and a sum of money, to Dr. Jacob E. Halm, late of the Royal Observatory, Blackford Hill, for his two papers on "Spectrographic Rotation of the Sun" and "Some Further Results obtained with the Spectroheliometer," and for other astronomical and mathematical papers published in the 'Proceedings' of the Society.

From an Oxford Note-Book.

Conferences bring the joy of new friendships and the consequent sorrow of losing friends. The glad memories of our recent visit to Paris have been saddened by the totally unexpected death of Trépied, whose genial and active presence at the meeting seemed to offer a guarantee against such a dire contingency, and whose last words to the writer were "Bien! au mois d'Octobre 1910." It was the first time he had been present at the Solar Union, but he had never missed a meeting of the Astrographic Conference or failed to be chosen as Secretary; and it is safe to say that no one else could have done the work so well. There were moments when his ready tact was the direct means of saving an awkward situation: one occasion especially will be remembered when a proposition was being pressed to a decision which Trépied at last pointed out was "at best useless, and at worst dangerous." It was severe criticism, and from other lips might have caused a conflagration rather than subdued one; but from Trépied, followed by the comical way in which he asked "What was the good of a resolution like that?" it did just what was wanted. How wonderfully and generously helpful he could be on special occasions those of us who had the good fortune to be invited to Algiers for the 1900 eclipse have good reason to know. He seemed to be a real part of the beautiful white observatory under the blazing blue sky, with the blue sea far below; and now, alas! it will know him no more. The hand of Death has of late been indeed heavy on French astronomy.

So, too, our visit to Vienna has a sad sequel in the death of an Oppolzer, one of those whom, like our own countryman Alexander Herschel, we instinctively regard as part of a great family rather than as individuals, however good their own work may have been. In the case of the Herschels the unsparing, almost reckless devotion to scientific work which characterized the founder remains as a family trait, and was certainly recognizable in the scion we have just lost; for Alexander Herschel was one of those who come under Prof. Hale's definition of amateurs—they work at science "because they can't help it."

And this brings us back to the brighter side of Conferences. The meetings of 1907 brought Prof. Hale over to these longitudes, and thereby the British Astronomical Association and the Royal Astronomical Society gained a first-rate lecture, and the University of Manchester a distinguished alumnus. I have been fortunate enough to obtain a copy of the nice things said at Manchester on June 29, and trust that they may be reproduced here without impropriety. There is no need to mention the name of the author, for it will be readily guessed:—

> In George Ellery Hale, a guest from the golden shores of California, we welcome an eminent astronomer and a distinguished administrator. It was at the early age of twenty-three that he first stepped into the arena of scientific investigation by designing the instrument which, in its recent form, gives us pictures of the Sun painted with pigments of metallic vapours, and enables us to probe its chemical and physical constitution to an extent surpassing the sanguine hopes of earlier spectroscopists. The famous Observatory of the University of Chicago, built and equipped by the late Mr. Yerkes, but conceived by Hale's energy, fertilized by his enthusiasm, and brought to fruition by his genius, will always remain a monument of splendid achievements obtained through an uncommon power of organization and the unselfish devotion of a chief to his fellow-workers.
>
> Our best wishes accompany him in the still more arduous undertaking which now lies before him, for, leaving the easier task of gathering the harvest of his labours, he has obeyed the call of his scientific brethren to direct the construction and organize the work of a vast solar laboratory on the crest of the Sierra Nevada.
>
> As an investigator his interests are wide, and his work is not that of a specialist, for the Sun to him is but a star placed conveniently near for detailed observation, and the stars are only landmarks in the history of cosmical growth. Yet while we admire the genius of the successful worker, we value as of greater worth the higher qualities of the man. Above all we desire to honour to-day the *noblesse* of his ideals and the straight path by which he has chosen to approach them.
>
> Mr. Vice-Chancellor, our University may already claim Hale as an alumnus,

for "Arduus ad Solem" is his device; to tie the link more closely on the anniversary of the day on which the Sun first shone upon him who now shines upon the Sun, I present to you George Ellery Hale for the degree of Doctor of Science.

WE have also been recently giving Honorary Degrees at Oxford. We have just elected as our new Chancellor of the University Lord Curzon, and on such occasions it is customary for the selection of recipients of our Doctorates to be made by the Chancellor himself. His list could certainly not be found fault with on the score of interest or variety, including as it did Prince Arthur of Connaught, General Booth, Mark Twain, and Rudyard Kipling, and, at the Chancellor's special request, undergraduates were re-admitted to the galleries of the Sheldonian Theatre at the ceremony, which was a return to a custom interrupted some years ago when it was thought they were becoming rather too noisy. Hence the new Doctors found cordial receptions awaiting them. Mark Twain, for instance, who had commented in public a few days before on the juxtaposition of two head-lines in the posters of the evening papers—"Mark Twain Arrives," "Ascot Cup Stolen"—was, of course, asked from the galleries: "Now what *did* you do with the Ascot Cup, Mark?" In the Chancellor's list there was one Astronomer, and the text of Prof. Love's oration was as follows:—

Inter eos qui solis stellarumque natura quæ sit optime docuerunt primarium fere locum occupat Josephus Norman LOCKYER. Dies me deficiat si miracula ab eo prolata de mundorum origine, de ratione quæ inter solis maculas et orbis terræ tempestates intercedat, si legationes solis defectus observandi causa in loca remotissima missas enumerare coner. Ex tot rebus gestis sumam duas tantum. Quadraginta abhinc annos duo viri, uterque suo marte fretus, Jannssen in Gallia, in Britannia hic noster, instrumenta effinxerunt quibus usi flammas illas ingentes, quas e sole excurrere videmus quando eius orbi luna officit, sole non obscurato cernerent. Cum ex harum flammarum observatione multum de materia e qua sol constat colligi possit, res tanti habita est ab Academia Gallica ut numismate impresso insigniretur. Altera huius viri laus est singularis quod Acta Hebdomadalia, in quibus quicquid novi in omni Scientiæ Naturalis genere a viris doctis ubique repertum est enarratur, conscribenda curavit: qua in re cum summam diligentiam et peritiam præstaret, de omnibus qui scientiæ promovendæ student optime meritus est.

I AM much indebted to a Harvard correspondent for the following interesting note:—

It might at first sight be thought that the City Fathers of Cambridge, Mass., were greatly opposed to Astronomy in general, and to the Harvard College Observatory in particular. Until recently, the Revised Ordinances of that City, Chapter 13, Section 73, read: "No structure or stand for observation purposes shall be constructed or occupied upon the roof of any building within the city." The entire income of the Observatory would be absorbed by the fines under this ordinance, but fortunately a change has been made in the wording in order that astronomical observations may be excepted. It may be mentioned incidentally that, to the average member of the City Government, "observation purposes" means erecting seats on top of your house if it overlooks a field on which the game of base-ball is played, so that you can sell to the public the opportunity to see the game without paying the usual price of admission or looking through a knot-hole in the high fence surrounding the grounds.

How the spectacle of genuine modesty warms the heart! The Seventeenth Annual Report of the Astronomer Royal for Scotland gives a vast amount of information, and yet is priced outside at one halfpenny! Criticism is disarmed, though we should have liked to enter a protest against the use of the term "tidal wave" in a scientific report. The publication of Henderson's Catalogue during the year is a notable event. Does anyone know of any existing portrait of Henderson? He was a very great man, but there seems to be no portrait to hand.

M. Deslandres distributed to us in Paris an article contributed by him to the 'Annuaire du Bureau des Longitudes' for 1907 entitled "Histoire des Idées et des Recherches sur le Soleil." I am sorry to see in it the following mis-statement, which occurs in a footnote on p. 134 of his separate pamphlet (C. 128 in the 'Annuaire'):—

> L'Institution Carnegie a accordé récemment une forte subvention au professeur Turner, pour la création d'un bureau de mesures, spécialement destinés aux recherches solaires. Le bureau sera adjoint à l'Observatoire que Turner dirige à Oxford.

Now this is unfortunately not the case. The only subsidy yet received is a sum of £250 from a private fund, quite unconnected with the Carnegie Institution. I am at a loss to think on what M. Deslandres can have founded his explicit statement, and that he should have made it without proper verification does not give us great confidence in the accuracy of his "history."

The last number of the Pub. Ast. Soc. Pacific gives the welcome news that the eclipse of next January will not pass without a strong attempt to observe it. A Lick Observatory Expedition will leave San Francisco on November 22 for Tahiti, whence a U.S. gunboat will take them on. Another expedition from the Smithsonian Institution will travel with them, and the double party will probably strain the limited resources to the fullest extent. Still it may be possible for some European enthusiast who could not otherwise see the eclipse to obtain assistance from this expedition. I hope it is not wrong to suggest to any such that he can do no harm by making enquiry of Prof. Campbell, for he will certainly be ready to help if it is in any way possible.

A correspondent writes:—"I see Barnard announces a vacancy in Taurus. Do you know who is applying for it?"

The following poem appeared recently in *Punch*, and we have not had any poetry lately:—

ARCTURUS.

["Arcturus, which is a gigantic orb equal to some twelve or thirteen hundred suns like our own, is flying through space at the rate of some 257 miles per second in a straight line for ever. . . . . If PTOLEMY were now alive it would require all his skill to perceive that Arcturus held a different position from that in which he used to study it."—*Mr. W. E. Garrett Fisher in 'The Tribune.'*]

ARCTURUS is whirled across space
In a race
With himself at a pace
Which takes him through heaven,
Professors have reckoned,
At two fifty-seven
Full miles to the second!

Just imagine him fizzing
And whizzing;
His size,
As he flies,
Is a mass of twelve hundred and more times the sun.
In an eager endeavour
He dashes for ever
Straight on with no possible object but fun.

For in vain is the pace of this monarch of stars,
Who has burst through the bars
That impeded his force:
In the years that have gone
He has hardly put on
Half an inch to the visible length of his course.

Now to fly and get on is no end of a game,
But to fly without moving, to stay in the same
Spot
Is not
What a planet with any respect
For himself and the fame
Of his name
Would select.

For if PTOLEMY saw him to-day
He would say,
"You're a run-away star,
But you haven't got far,
And, forgive the remark, you were just where you are
When I saw you some thousands of seasons ago.
For a star of your parts you're confoundedly slow."

What the deuce
Is the use
Of this fever and fuss,
If Arcturus is still so absurdly like us;
If, in spite of his chances,
He never advances;
If he breaks all the records for scurry and fizz,
But with all his impatience remains where he is?

And yet he is whirled across space
In a race
With himself at a pace
Which takes him through heaven,
Professors have reckoned,
At two fifty-seven
Full miles to the second!

TIB.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXX. SEPTEMBER, 1907. No. 387.

MEETING OF THE BRITISH ASSOCIATION AT LEICESTER.

Address by the President, Sir David Gill, K.C.B., LL.D., D.Sc., F.R.S., Hon.F.R.S.E., &c., 1907, *July* 31.

[Continued from p. 306.]

The Distribution of Varieties of Luminosity of Stars.

But, besides the mean parallax of stars of a particular magnitude and proper motion, it is essential that we should know approximately what percentage of the stars of such a group have twice, three times, &c., the mean parallax of the group, and what percentage only one-half, one-third of that parallax, and so on. In principle, at least, this frequency-law may be obtained by means of the directly determined parallaxes. For the stars of which we have reliable determinations we can compare these true parallaxes with the *mean* parallax of stars having corresponding magnitude and proper motion, and this comparison will lead to a knowledge of the frequency-law required. It is true that, owing to the scarcity of material at present available, the determination of the frequency-law is not so strong as may be desirable, but further improvement is simply a question of time and the augmentation of parallax-determination.

Adopting provisionally the frequency-law found in this way by Kapteyn *, we can localize all the stars in space down to about the ninth magnitude.

Take, for example, the stars of magnitude 5·5 to 6·5. There are about 4800 of these stars in the whole sky. According to Auwers-Bradley, about 9½ per cent. of these stars, or some 460 in all, have proper motions between 0″·04 and 0″·05. Now, according to Kapteyn's empiric formula, whose satisfactory agreement with the Yale results has just been shown, the mean parallax of such stars is almost exactly 0″·01. Further, according to his frequency-law, 29 per cent. of the stars have parallaxes between the *mean* value and double the mean value; 6 per cent. have parallaxes between twice and three times the mean value; 1½ per cent. between three and four times the mean value. Therefore, of our 460 stars 133 will have parallaxes between 0″·01 and 0″·02, 28 between 0″·02 and 0″·03, 7 between 0″·03 and 0″·04, and so on.

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* Publications Astron. Lab. Groningen, No. 8, p. 23.

Localizing in the same way the stars of the sixth magnitude having other proper motions, and then treating the stars of the first magnitude, second magnitude, third magnitude, and so on to the ninth magnitude in the same way, we finally locate all these stars in space *.

It is true we have not localized the individual stars, but we know approximately and within certain limits of magnitude the number of stars at each distance from the Sun.

Thus the apparent brightness and the distance being known we have the means of determining the light-energy or absolute *luminosity* of the stars, *provided it can be assumed that light does not suffer any extinction in its passage through interstellar space.*

Consider, lastly, the distribution of stellar density, that is, the number of stars contained in the unit of volume.

We cannot determine *absolute* star-density, because, for example, some of the stars which we know from their measured parallaxes to be comparatively near to us are in themselves so little luminous that if removed to even a few light-years greater distance they would appear fainter than the ninth magnitude, and so fall below the magnitude at which our data at present stop.

But if we assume that intrinsically faint and bright stars are distributed in the same proportion in space, it will be evident that the comparative richness of stars in any part of the system will be the same as the comparative richness of the same part of the system in stars of a particular luminosity. Therefore, as we have already found the arrangement in space of the stars of different degrees of luminosity, and consequently their number at different distances from the Sun, we must also be able to determine their relative density for these different distances.

Kapteyn finds in this way that, starting from the Sun, the star-density (*i.e.* the number of stars per unit volume of space) is pretty constant till we reach a distance of some 200 light-years. Thence the density gradually diminishes till, at about 2500 light-years, it is only about *one-fifth* of the density in the neighbourhood of the Sun †. This conclusion must, however, be regarded as uncertain until we have by independent means been enabled to estimate the absorption of light in its course through interstellar space, and obtained proof that the ratio of intrinsically faint to bright stars is constant throughout the universe.

Thus far Kapteyn's researches deal with the stellar universe as a whole; the results, therefore, represent only the *mean* conditions of the system. The further development of our knowledge demands a like study applied to the several portions of the universe separately. This will require much more extensive material than we at present possess.

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* Publications Astron. Lab. Groningen, No. 11, Table II.

† *Ibid.* No. 11.

As a first further approximation the investigation will have to be applied separately to the Milky Way and the parts of the sky of higher galactic latitude. The velocity and direction of the Sun's motion in space may certainly be treated as constants for many centuries to come, and these constants may be separately determined from groups of stars of various regions, various magnitudes, various proper motions, and various spectral types. If these constants as thus separately determined are different, the differences which are not attributable to errors of observation must be due to a common velocity or direction of motion of the group or class of star to which the Sun's velocity or direction is referred. Thus, for example, the Sun's velocity as determined by spectroscopic observations of motion in the line of sight appears to be sensibly smaller than that derived from fainter stars. The explanation appears to be that certain of the brighter stars form part of a cluster or group of which the Sun is a member, and these stars tend to some extent to travel together. For these researches the existing material, especially that of the determination of velocities in the line of sight, is far too scanty.

Kapteyn has found that stars whose proper motions exceed 0″·05 are not more numerous in the Milky Way than in other parts of the sky *; in other words, if only the stars having proper motions of 0″·05 or upwards were mapped there would be no aggregation of stars showing the existence of a Milky Way.

The proper motions of stars of the second spectral type are, as a rule, considerably larger than those of the first type; but Kapteyn comes to the conclusion that this difference does not mean a real difference of velocity, but only that the second-type stars have a smaller luminosity, the mean difference between the two types amounting to 2½ magnitudes †.

But for the solution of the riddle of the universe much more is required. Besides the proper motions, which would be derived from the data just described, we need for an ideal solution to know the velocity in the line of sight, the parallax, the magnitude, and the spectrum-type of every star.

The broad distinction between these latter data and the determination of proper motion is this, that whereas the observations for proper motion increase in value as the square of their age, those for velocity in the line of sight, parallax, magnitude, and type of spectrum may, for the broader purposes of cosmical research, be made at any time without loss of value. We should therefore be most careful not to sacrifice the interests of the future by immediate neglect of the former for the latter lines of research. The point is that those observatories which undertake this meridian work should set about it with the least possible delay, and prosecute the programme to the end with all possible zeal. Three observatories in each hemisphere should be sufficient; the quality of the work should be of the best, and quality should not be sacrificed for speed of work.

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* Verl. Kn. Akad. Amsterdam, January 1893.

† *Ibid.* April 1892.

But the sole prosecution of routine labour, however high the ultimate object, would hardly be a healthy condition for the astronomy of the immediate future. The sense of progress is essential to healthy growth, the desire to know must in some measure be gratified. We have to test the work that we have done in order to be sure that we are working on the right lines, and new facts, new discoveries, are the best incentives to work.

For these reasons Kapteyn, in consultation with his colleagues in different parts of the world, has proposed a scheme of research which is designed to afford within a comparatively limited time a great augmentation of our knowledge. The principle on which his programme is based is that adequate data as to the proper motions, parallaxes, magnitudes, and the type of spectrum of stars situated in limited but symmetrically distributed areas of the sky, will suffice to determine many of the broader facts of the constitution of the universe. His proposals and methods are known to astronomers and need not therefore be here repeated. In all respects save one these proposals are practical and adequate, and the required co-operation may be said to be already secured—the exception is that of the determination of motion in the line of sight.

All present experience goes to show that there is no known satisfactory method of determining radial velocity of stars by wholesale methods, but that such velocities must be determined star by star. For the fainter stars huge telescopes and spectroscopes of comparatively low dispersion must be employed. On this account there is great need in both hemispheres of a huge reflecting telescope—six to eight feet in aperture—devoted almost exclusively to this research. Such a telescope is already in preparation at Mount Wilson, in America, for use in the northern hemisphere. Let us hope that Prof. Pickering's appeal for a large reflector to be mounted in the southern hemisphere will meet with an adequate response, and that it will be devoted there to this all-important work.

Conclusion.

The ancient philosophers were confident in the adequacy of their intellectual powers alone to determine the laws of human thought and regulate the actions of their fellow men, and they did not hesitate to employ the same unsupported means for the solution of the riddle of the universe. Every school of philosophy was agreed that some object which they could see was a fixed centre of the universe, and the battle was fought as to what that centre was. The absence of facts, their entire ignorance of methods of exact measurement, did not daunt them, and the question furnished them a subject of dispute and fruitless occupation for twenty-five centuries.

But astronomers now recognize that Bradley's meridian observations at Greenwich, made only 150 years ago, have contributed more to the advancement of sidereal astronomy than all the speculations of preceding centuries. They have learned the lesson that human knowledge in the slowly developing phenomena of sidereal astronomy must be content to progress by the accumulating labours of successive generations of men; that progress will be measured for generations yet to come more by the amount of honest, well-directed, and systematically discussed observation than by the most brilliant speculation; and that, in observation, concentrated systematic effort on a special thoughtfully selected problem will be of more avail than the most brilliant but disconnected work.

By these means we shall learn more and more of the wonders that surround us, and recognize our limitations when measurement and facts fail us.

Huggins's spectroscope has shown that many nebulæ are not stars at all; that many well-condensed nebulæ, as well as vast patches of nebulous light in the sky, are but inchoate masses of luminous gas. Evidence upon evidence has accumulated to show that such nebulæ consist of the matter out of which stars (*i. e.* suns) have been and are being evolved. The different types of star-spectra form such a complete and gradual sequence (from simple spectra resembling those of nebulæ onwards through types of gradually increasing complexity) as to suggest that we have before us, written in the cryptograms of these spectra, the complete story of the evolution of suns from the inchoate nebula onwards to the most active sun (like our own), and then downward to the almost heatless and invisible ball. The period during which human life has existed on our globe is probably too short—even if our first parents had begun the work—to afford observational proof of such a cycle of change in any particular star; but the fact of such evolution, with the evidence before us, can hardly be doubted. I most fully believe that, when the modifications of terrestrial spectra under sufficiently varied conditions of temperature, pressure, and environment have been further studied, this conclusion will be greatly strengthened. But in this study we must have regard also to the spectra of the stars themselves. The stars are the crucibles of the Creator. There we see matter under conditions of temperature and pressure and environment, the variety of which we cannot hope to emulate in our laboratories, and on a scale of magnitude beside which the proportion of our greatest experiment is less than that of the drop to the ocean. The spectroscopic astronomer has to thank the physicist and the chemist for the foundation of his science, but the time is coming—we almost see it now—when the astronomer will repay the debt by wide-reaching contributions to the very fundamenta of chemical science.

On a Method of Determining Latitude, Azimuth, and Time free from the Errors of Refraction.

(1) A METHOD of reaching position without correcting for refraction may possibly be of some interest beyond the field of its application as at present suggested. That field was limited by the following considerations: (1) it is desirable that students should start at an early stage of their astronomical work with the careful use of the theodolite for determination of position; (2) they begin this work long before they come to the intelligent study of refraction corrections; (3) even if they have been taught to use refraction corrections without knowledge of any theory, such corrections are often wide of the mark when they are applied in the heart of a big city. Accordingly, in the course of theodolite practice at University College, London, it seemed desirable to devise a method of determining position which should not need any refraction corrections. The method may, we think, be of service to others who have to work under similar limitations, or who are doubtful as to refraction under special atmospheric conditions.

(2) The method is briefly as follows:—The theodolite being levelled and the azimuth zero-mark determined, the telescope is clamped in a given azimuth, and the times of transit of two known stars over the vertical hair are observed by means of a chronometer of known rate. The telescope is now unclamped and turned through a known angle in azimuth, reclamped, and the times of transit of the same two stars across the new position of the vertical hair observed. These are all the observations requisite to determine the latitude and meridian of the instrument and the clock-error. For educational purposes it is as well that the student should read the altitudes of the stars at transit, if only to be absolutely certain that he has, in sweeping with his vertical circle, got the transits of the *desired* stars; these altitudes can only be read with one face as it is not desirable to unclamp the telescope in azimuth between the transits.

(3) Let the declinations and right ascensions of the two stars be δ_1, $Æ_1$, δ_2, and $Æ_2$, the "first" star being the first to cross the vertical hair in the first position, which shall correspond to angle α, from the azimuth mark. Let the times of transit be t_1 and t_2, h_1 and h_2 the hour-angles supposed east at transit over the first position of the hair, A the azimuth of this position, and λ the latitude. A and λ are to be found. Let

$$\chi = Æ_2 - Æ_1 - (t_2 - t_1),$$

then

$$\chi = h_2 - h_1,$$

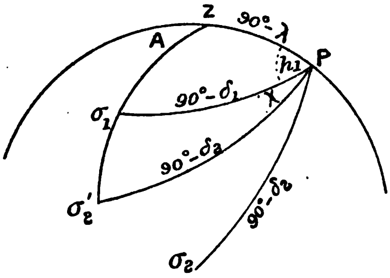
the difference of the hour-angles at transit, supposing $\delta_2 < \delta_1$. If, however, $\delta_2 > \delta_1$, the star which transits later has the lesser hour-angle at transit; in this case

$$h_1 - h_2 = \text{\AE}_1 - \text{\AE}_2 + t_2 - t_1$$

is positive, and we will call this χ. Thus χ is to be taken the positive arithmetical value of

$$(\text{\AE}_2 - \text{\AE}_1) = (t_2 - t).$$

Let Z be zenith, P the pole, σ_1 and σ_2 the two stars at transit of the first star, and σ_2' the second star at transit.



From the triangle $ZP\sigma_1$:

$$\tan\delta_1 = \tan\lambda\cos h_1 - \sin h_1 \sec\lambda\cot A \quad . \quad . \quad . \quad \text{(i)}$$

and from the triangle $ZP\sigma_2'$:

$$\tan\delta_2 = \tan\lambda\cos h_2 - \sin h_2 \sec\lambda\cot A \quad . \quad . \quad . \quad \text{(ii)}$$

Hence:

$$\tan\delta_1\cos h_2 - \tan\delta_2\cos h_1 = \sin(h_2 - h_1)\cot A\sec\lambda,$$
$$\tan\delta_1\sin h_2 - \tan\delta_2\sin h_1 = \sin(h_2 - h_1)\tan\lambda.$$

Square these equations, add them, and add

$$\sin^2(h_2 - h_1) = \sin^2\chi$$

to both sides of the result, then

$$\tan^2\delta_1 + \tan^2\delta_2 - 2\tan\delta_1\tan\delta_2\cos\chi + \sin^2\chi$$
$$= \sin^2\chi\,\text{cosec}^2 A\sec^2\lambda \quad . \quad . \quad \text{(iii)}$$

Now

$$1/Q^2 = \frac{\tan^2\delta_1 + \tan^2\delta_2 - 2\tan\delta_1\tan\delta_2\cos\chi + \sin^2\chi}{\sin^2\chi} \quad . \quad \text{(iv)}$$

is a known quantity; thus we find

$$\sin A = \pm Q\sec\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(v)}$$

The positive sign is to be taken if the observations be taken over the first position of the vertical hair east of south *.

Now let the theodolite be turned in azimuth through an angle β, and let

$$1/Q'^2 = \frac{\tan^2\delta_1 + \tan^2\delta_2 - 2\tan\delta_1\tan\delta_2\cos\chi' + \sin^2\chi'}{\sec^2\chi'} \quad . \quad \text{(vi)}$$

where χ' is the arithmetical value of

$$(Æ_2 - Æ_1) - (t_2' - t_1'),$$

t_1' and t_2' being the times of transit of the two stars over the second position of the hair. We have

$$\sin A' = \sin(A - \beta) = Q' \sec\lambda \quad . \quad . \quad . \quad . \quad \text{(vii)}$$

Here if β be $>$ A, *i. e.* if the second observation be west of south, Q′ must be given the negative sign, (v) and (vii) give us

$$\sin(A - \beta)/\sin A = Q'/Q,$$

$$\cos\beta - \sin\beta\cot A = Q'/Q,$$

or

$$\sin A = \frac{Q\sin\beta}{\sqrt{Q^2 + Q'^2 - 2QQ'\cos\beta}} \quad . \quad . \quad . \quad \text{(viii)}$$

and from (v)

$$\cos\lambda = \frac{\sqrt{Q^2 + Q'^2 - 2QQ'\cos\beta}}{\sin\beta} \quad . \quad . \quad . \quad . \quad \text{(ix)}$$

These formulæ hold if both vertical circles are on the same side of the meridian; if they are on opposite sides of the meridian, then Q′ must be given the negative sign, or the radical written

$$\sqrt{Q^2 + Q'^2 + 2QQ'\cos\beta}.$$

If the theodolite be turned exactly through a right angle,

$$\sin A = Q/\sqrt{Q^2 + Q'^2}, \quad \cos\lambda = \sqrt{Q^2 + Q'^2},$$

which suggest the possibility of using a simple instrument without any graduation in altitude or azimuth, but adjusted with two stops to turn accurately through a right angle.

Returning to Equation (i) take

$$\tan\psi = \cot A \operatorname{cosec}\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(x)}$$

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* An indication of these formulæ was given by me in a question provided for the B.Sc. Examination in Astronomy in the University of London in 1905.—K. P.

and we find

$$\tan\delta_1 = \tan\lambda\,(\cos h_1 - \tan\psi\sin h_1),$$

or

$$\cos(h_1+\psi) = \tan\delta_1\cos\psi\cot\lambda \quad . \; . \; . \; . \; . \quad \text{(xi)}$$

Thus the hour-angle of the first star at first transit is obtained and the error of the clock ascertained.

(4) It seems desirable to illustrate the work on an actual series of observations. The observations were made by Mr. T. Ormsby, Lecturer on Surveying in University College, assisted by Mr. Everitt, the instrument used being a small theodolite, readings on the horizontal circle being certainly not capable of greater accuracy than 5″, and the level only graduated to 10″. The date was March 6, 1907. The stars selected were Castor and Procyon, the first vertical circle being towards the south-east and the second roughly south-west. For the first vertical circle Procyon was the "first" and Castor the "second" star, the order of transit being reversed at the second position of the vertical hair. The accompanying table gives the needful data.

Observations of Castor and Procyon, March 6, 1907.

Star.	Time by Sidereal Clock*.	Declination.	R.A.	Horizontal Angle.
	h m s	° ′ ″	h m s	° ′ ″
Procyon ...	5 41 1·0	5 27 38·7	7 34 26·5	230 36 40
Castor	6 25 23·3	32 5 34·0	7 28 40·5	
Castor	8 13 27·8	32 5 34·0	7 28 40·5	294 38 55
Procyon ...	8 55 5·5	5 27 38·7	7 34 26·5	

Castor being the second star at first transit,

$$t_2 - t_1 = 44^{m}\ 22^{s}{\cdot}3 \quad\text{and}\quad Æ_2 - Æ_1 = -(5^{m}\ 46^{s}{\cdot}0).$$

Thus

$$Æ_2 - Æ_1 - (t_2 - t_1) = -(50^{m}\ 8^{s}{\cdot}3);$$

but since δ_2 is $> \delta_1$, this must be given the positive sign, or

$$\chi = 50^{m}\ 8^{s}{\cdot}3 = 12^{\circ}\ 32'\ 4''{\cdot}5.$$

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* Actually the readings were taken by chronometer compared with sidereal clock.

We find

$$\left.\begin{array}{l}\tan^2\delta_1 = {\cdot}393{,}283\\ \tan^2\delta_2 = {\cdot}009{,}139\\ \sin^2\chi = {\cdot}047{,}102\end{array}\right\} = {\cdot}449{,}524,$$

$$2\tan\delta_1\tan\delta_2\cos\chi = \qquad {\cdot}117{,}045,$$

$$u^2 = \tan^2\delta_1+\tan^2\delta_2-2\tan\delta_1\tan\delta\cos\chi+\sin^2\chi = {\cdot}332{,}479.$$

Whence

$$\log Q = \log\sin\chi-\log u = \bar{1}{\cdot}575{,}6354.$$

At the second vertical circle, Castor is the first star, and accordingly

$$\text{Æ}_1 = 7^{\text{h}}\,28^{\text{m}}\,40^{\text{s}}{\cdot}5,\quad \text{Æ}_2 = 7^{\text{h}}\,34^{\text{m}}\,26^{\text{s}}{\cdot}5.$$

Thus

$$\text{Æ}_4-\text{Æ}_1 = 5^{\text{m}}\,46^{\text{s}}{\cdot}0,\quad \text{while}\quad t_3-t_1 = 41^{\text{m}}\,37^{\text{s}}{\cdot}7.$$

Accordingly

$$\text{Æ}_2-\text{Æ}_1-(t_2-t_1) = -(35^{\text{m}}\,51^{\text{s}}{\cdot}7).$$

This negative value of χ' will not affect the numerical value of Q'. We have in degrees, &c.,

$$\chi' = -(8^\circ\,57'\,55''{\cdot}5).$$

Thus

$$\left.\begin{array}{l}\tan^2\delta_1 = {\cdot}009{,}139\\ \tan^2\delta_2 = {\cdot}393{,}283\\ \sin^2\chi' = {\cdot}024{,}285\end{array}\right\} = {\cdot}426{,}708,$$

$$2\tan\delta_1\tan\delta_2\cos\chi' = \qquad {\cdot}118{,}438,$$

$$u'^2 = \tan^2\delta_1+\tan^2\delta_2-2\tan\delta_1\tan\delta_2\cos\chi'+\sin^2\chi' = {\cdot}308{,}270.$$

Whence

$$\log(\pm Q' = \log\sin(-\chi')-\log u' = \bar{1}{\cdot}448{,}2086.$$

Here Q' must be given the negative sign, as the second vertical circle was west of the meridian and accordingly the azimuth negative.

We find accordingly

$$Q = +{\cdot}376{,}388,$$
$$Q' = -{\cdot}280{,}678.$$

We next proceed to determine $Q^2+Q'^2-2QQ'\cos\beta$.

$$\beta = 64^\circ\,2'\,15''.$$

$$\begin{array}{r}Q^2 = {\cdot}141{,}668\\ Q'^2 = {\cdot}078{,}780\\ -2QQ'\cos\beta = \underline{{\cdot}092{,}498}\end{array}$$

$$v^2 = Q^2+Q'^2-2QQ'\cos\beta = {\cdot}312{,}946$$

$$\log\cos\lambda = \log v-\log\sin\beta = \bar{1}{\cdot}793{,}9360.$$

Whence *

$$\lambda = 51^\circ\ 31'\ 21''\cdot4.$$

Again,

$$\log \sin A = \log Q - \log \cos \lambda = \bar{1}\cdot781{,}6994,$$

or $$A = 37^\circ\ 13'\ 24''.$$

Thus from the zero reading mark of horizontal circle to meridian south was

$$37^\circ\ 13'\ 24'' + 230^\circ\ 36'\ 40'' = 267^\circ\ 40'\ 4''.$$

Or, from zero reading mark to the north meridian measured clockwise

$$= 87^\circ\ 50'\ 4''.$$

We can now proceed to determine the time at first transit by (x) and (xi). Confining our attention to Procyon we have

$$\log \tan \psi = \log \cot 37^\circ\ 13'\ 24'' - \log \sin 51^\circ\ 31'\ 21''\cdot4,$$

whence $$\psi = 59^\circ\ 15'\ 33''\cdot0.$$

Next,

$$\log \cos (h_1 + \psi) = \log \tan \delta_1 + \log \cos \psi + \log \cot \lambda$$

gives $$h_1 + \psi = 87^\circ\ 46'\ 27''\cdot3,$$

or $$h_1 = 28^\circ\ 30'\ 54''\cdot3$$
$$= 1^h\ 54^m\ 3^s\cdot6.$$

Thus

$$\text{true sidereal time} = 7^h\ 34^m\ 26^s\cdot5 - 1^h\ 54^m\ 3^s\cdot6$$
$$= 5^h\ 40^m\ 22^s\cdot9.$$

The clock was thus $38^s\cdot1$ fast.

Taking Procyon at the second vertical circle transit, we find

$$\psi = 68^\circ\ 24'\ 42''\cdot3,$$

whence $$h_1' + \psi = 88^\circ\ 24'\ 52''\cdot9,$$

and $$h_1' = 20^\circ\ 0'\ 10''\cdot6$$
$$= 1^h\ 20^m\ 0^s\cdot7.$$

Thus

$$\text{true sidereal time} = h_1' + Æ = 8^h\ 54^m\ 27^s\cdot2.$$

Or, at the second transit, the clock was $38^s\cdot3$ fast. Thus the clock-rate was $1^s\cdot5$ a day.

(5) The example seems to indicate that, with more accurate levelling than is possible with a small theodolite, results might be attained as free from the errors of refraction as those of a zenith

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* Within $5''\cdot5$ of the true latitude, as close as it is probable to set with *one* determination and a theodolite of the size used.

telescope, and probably as precise. Further, they would have all the advantages which arise from using "clock stars," and thus saving considerable reduction. For repeated observations the arithmetical work is much simplified by using the same pairs of stars and the same horizontal angles β. Further, Equations (viii) and (ix) show that β should be fairly large, and if Q and Q' are of opposite sign, *i. e.* on different sides of the meridian, the error of the observations is likely to be smaller. In such a case also the error of level will be in a general way reduced. An interval of three or four hours is not an inconveniently long period to allow between the observations, and a series of observations on pairs of clock-stars can be made in the course of the evening. It does not, however, seem desirable to move the theodolite in azimuth between the transits of the two members of the pair over the same position of the vertical hair. We publish this method as a convenient exercise for students in theodolite practice; but we venture to think that a small transit telescope mounted so as to rotate in azimuth with stops, say, at 0°, 45°, and 90°, but otherwise with no graduation at all, might prove an economical and effective instrument for large surveys.

KARL PEARSON, T. ORMSBY.

P.S.—When writing this paper, I was unaware that a good deal of work had been done apart from Talcott's method on determining the latitude, azimuth, and time from theodolite or altaz observations free of the error of refraction. The Editors have kindly referred me to Döllen, Cooke, and Wade. Döllen * observes in the vertical of the pole-star, and introduces elaborate tables to expedite the reduction work. Cooke † and Wade ‡ observe the transit of known stars over a fixed horizontal small circle. The latter method seems of very great value, and it will be of interest to compare the results obtained by the horizontal circle with the present method for the vertical circle in rough field-work. None of the three writers cover the same ground as our Q method. But a fourth writer distinctly has done so, namely Houzeau §, in a memoir of 1853, which I picked up from a recent second-hand book catalogue. Houzeau's analysis is very different from mine; he introduces two auxiliary angles, one depending on the position of the vertical of observation, and one on the first transiting star. I do not feel certain that his method is preferable. Thus he does not use the angle β between the two verticals of observation, but the interval of time between the transit of the first star over the first vertical and the first star over the second vertical. This may be a considerable interval—three or four hours—and demands a known and uniform clock-rate. It is accordingly a question whether it is better to trust the clock or the horizontal circle of the altaz. Notwithstanding this and other differences of treatment, the method by double transits over two verticals to find latitude, azimuth, and time is essentially that of Houzeau, and the present paper only claims to recall that method in a different form for the purposes of field astronomy.—K. P.

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* 'Die Zeitbestimmung vermittelst des tragbaren Durchgangsinstruments im Verticale des Polarsterns Festschrift.' Pulkowa, 1863.

† "On a New and Accurate Method of Determining Time, Latitude, and Azimuth with a Theodolite." M. N. R. A. Soc. vol. lxiii. pp. 156–164 (1903), and vol. lxiv. p. 70 (additional corrective note).

‡ "Remarks on a Paper by Mr. Cooke on a New Method, &c." M. N. R. A. Soc. vol. lxiv. pp. 107–112 (1904).

§ "Méthode pour déterminer simultanément la latitude, la longitude, l'heure, et l'azimut, par des passages observés dans deux verticaux." Académie royale de Belgique, t. xxv. Mémoires des savants étrangers, pp. 1–26 (1853).

Father Hagen's Variable Star Atlas.

"ATLAS Stellarum Variabilium divisus est in quinque Series, quarum tres priores ad observandas stellas variabiles tenuissimam lucem attingentes adjumento sunt, quarta vero ad illas, quarum lux minima instrumentis mediocribus patet, quinta denique ad reliquas, quæ nudis oculis conspicuæ manent."

How these opening words of the Prefatio to Part I. bring back one's school-days and Cæsar's 'De Bello Gallico'! "Gallia est omnis divisa in partes tres, quarum unam incolunt Belgæ, &c." Who that encountered these words in the first of a long series of dire struggles with the Latin tongue can ever forget them? But Time mellows even the sorrows of childhood into kindly remembrances, and the words come to have at the last a place almost in our affections, especially when there comes with the memory of them the curious experience of finding Latin almost translatable at sight. "Atque hæc de Chartis jam Catalogum explicemus." There does not seem to be any difficulty? Can this be the same Latin which refused to yield any grain of sense or meaning, though we picked out all possible equivalents for each word and shuffled the order like a pack of cards many times over? "Scala hæc ita divisa est in decem partes"—our old friend All Gaul again in a new dress! But perhaps we are deceiving ourselves as to the facility by unconsciously picking out the easy bits; if the old discipline were reinstated, and we were put on to construe at an unexpected place, we might be back in the old troubles. "Ex Collegio Georgiopolitano"—there's a hard word; where are the dictionary and the atlas—the geographical atlas?

Those who, like the Order to which Father Hagen belongs, have not ceased to use Latin, will be amused to learn how strange it appears to some in a modern astronomical work. It is as though we were suddenly confronted with a fragment of a bygone age, as were the huntsmen in Stevenson's charming story :—

> And there is an old tale enhances for the imagination the grandeur of the woods of France. . . When Charles VI. hunted in the time of his wild boyhood

near Senlis, there was captured an old stag, having a collar of bronze about his neck, and these words engraved on the collar: "Cæsar mihi hoc donavit." It is no wonder if the minds of men were moved at this occurrence and they stood aghast to find themselves thus touching hands with forgotten ages.

Whether this is meant as a serious piece of history or a fairy tale one can well afford never to know: it is in any case so striking a picture that it cannot fail to distract Father Hagen's attention from any undue levity in the foregoing remarks.

Moreover they are not purely irrelevant. I submit that the work under review inevitably reminds us, not only by the language of its preface, which is after all only a detail, but by its fundamental form, of a gap between past and present, though the past may not be very distant. Hagen's Variable Star Atlas represents a painstaking, laborious piece of work done by eye and hand in a field where we have lately come to regard photography as the obvious method; for the measures are differential throughout, and though photographic processes have not as yet taken their proper place in meridian astronomy, their pre-eminence for differential work would be generally admitted.

Father Hagen had, however, begun his arduous labours before the modern regime; in the early nineties the photographic method was still in the experimental stages, and, however good its prospects of future success may have been, the moment was not one at which to abandon a project already well advanced for an uncertain improvement. If this were all, we should be content to admire the energy which carried the work to a conclusion in face of the obvious discouragements due to threatened supersession. If this were all, Father Hagen would still be the first man to make a whole voyage which others had only commenced, and it would be all the more creditable to him that he had done it in a sailing-ship at the moment when steam was going to reduce the difficulties.

But it is not all. It does not seem at all impossible that for the particular voyage he has just completed sails and not steam may be the better equipment, at any rate for a large part of the way. If we desire to get accurate relative *positions* of the stars within a limited region, the photographic method is almost certainly the best; but it is the relative *magnitudes* of the stars, and not their positions, which are important in an Atlas of Variable Stars. Now no one who has tried to get accurate magnitudes from photographs will as yet be eager to urge the claims of the photographic over visual methods, especially when it is remembered that (*a*) the magnitudes are wanted for use in *visual* work, (*b*) those of the faint stars should be as accurate as those of the bright. When these points are borne clearly in mind, it will be obvious that a large part of the work of forming the Atlas must be done at the telescope, though photographs might have helped in determining positions. The latter, however, need not be very accurate; all that is necessary being to secure identification. Thus Father Hagen may be by no means the last sailor; we may still hope to see plenty of beautiful white canvas, notwithstanding the greater speed of the black smoke.

The voyage on which he embarked some fifteen years ago is at last happily and prosperously concluded, and he reverently glorifies God for his safe return to port:—

> Faxit Deus, quo magis in dies cæli enarrent gloriam suam, ut hoc Atlante via paretur ad stellarum variabilium arcana altius investiganda, plenius intellegenda.

On rising from his knees he may well look for heartly congratulation from his fellow astronomers, and grateful thanks from those in whose interests he has toiled for so many years. Some of these cannot thank him at the moment, because they are not yet born as variable-star observers—perhaps not yet born in the actual flesh. Others who would thank him have passed away. I well remember my first introduction to the Atlas, which was also my first meeting with the late Sir Cuthbert Peek. The occasion was some visit to the R.A.S. Library, for one of the thousand different purposes which draw one thither, and I found Sir Cuthbert Peek turning over the leaves of the newly-arrived instalment of the Atlas with the careful fingers of a man who well knew its worth. And I am tempted to add that it is not only the observers of variables who are in debt to the maker of the Atlas; there are others who have had to collate old observations, who find one chart substituted for another with only internal evidence to tell of the change; who find two observers professing the same chart and giving inconsistent readings; who find the same observer using as many as sixteen different charts for the same star—men who have had such experiences know the value of Father Hagen's work if only observers will, for the future, generally adopt it; and such men meditate, somewhat sorrowfully it may be, on the labour and uncertainty that might have been saved if Pogson had been able to carry through the similar project on which he started half a century ago.

It could be wished, in the interests of its general use, that the Atlas were not so costly. Variable-star observers are not in general rich men, and it is to be feared that many of them will be quite unable to buy the complete set of 5 parts. Is it still possible to arrange for the sale of separate sheets, even at a relatively greater cost? The advantages of universal reference to the same standard are obvious, but they cannot be secured unless the standard is accessible to all.

Another thought naturally arises. Could not the work be extended to the South Pole by the same eye and mind which have carried it so far? It seems ungrateful to ask a man just returned from a long voyage to set forth on a similar undertaking at once, and we know that Father Hagen's energies have been claimed for another enterprise—to wit, the Vatican share of the Astrographic Catalogue. But there is a point of view from which the interests of astronomy would be better served by his completing this great work for the whole sky and leaving to others the barely commenced new departure. Whether this point of view is still tenable is for others to decide; but it is hoped that, in the general interest, the question may be respectfully asked without danger of misunderstanding. The idea of enabling a man who has done a considerable piece of work for one hemisphere to extend it to the other is an attractive one, and has recently been favourably considered by the Carnegie Institution. It is, for instance, understood that when Prof. Boss finds his splendid piece of work on the collation of star catalogues sufficiently far advanced to set him free, facilities will be given him for taking his carefully-studied meridian-circle to the Southern Hemisphere and extending his northern observations to the whole sky. But as yet the scheme is only a possibility. Again, Prof. E. C. Pickering has urged the desirability of extending the Cape Photographic Durchmusterung to the Northern Hemisphere (a curious inversion of the usual order of things), and though some of us do not feel that we can wholeheartedly support this particular suggestion, it is quite possible that Prof. Pickering is in the right. As yet, however, the scheme is again only suggested. Unless I am mistaken, the late Mr. McClean was the first astronomer to extend in person a definite survey over both hemispheres, and Father Hagen has an excellent opportunity to be the second.

Series IV., the last of the five parts to appear, contains charts for 100 variables, with particulars of from 20 to 60 comparison stars for each. It is devoted to variables which remain within the limits of the Bonn Durchmusterung, both in position and magnitude. This instalment is accordingly larger than the others and correspondingly more expensive, costing £6 5*s*., while the others average about 5½ guineas. These prices are, as already remarked, rather serious for most of our pockets, though the beautiful workmanship is undoubedly well worth the sum asked. Indeed it is to be feared that the publisher (Mr. F. L. Dames, of Berlin) may not find his enterprise altogether profitable even at these figures, unless all such astronomical libraries that can afford to do so will make a point of obtaining the volumes.

The value of Parts V. and IV. has been greatly enhanced by the adoption of the Harvard photometric scale for the comparison stars. This was not possible for Parts I. to III. at the time when they were published; perhaps a series of corrected magnitudes can be given now?

It seems scarcely necessary to comment on other details, which are generally similar to those of the preceding parts. The adopted colour-scale (numbered 1–10) is now interpreted in terms of the Potsdam W, GW, WG, G, &c.; and attention may be called in passing to the important discussion of the effect of colour on estimation of brightness which has just appeared in the Introduction to the Potsdam Photometric Durchmusterung. The

"Purkinje phenomenon" is therein demonstrated quantitatively in a beautiful manner.

The occasion of the completion of this noble piece of work should not be allowed to pass without a tribute to the memory of Miss C. W. Bruce, of New York, whose generosity is acknowledged on the titlepage to each part. Nor should we omit to remark, in admiring the personal achievement of Father Hagen, that his devotion has a wider significance. It is one more instance of the devotion to Astronomy of the Order to which he belongs. Without the Jesuits we should miss many noble chapters from astronomical history. H. H. TURNER.

The Sun-spots of 1907 *May.*

As April was a quieter month than March, so far as spot-disturbance on the Sun is concerned, so May was quieter than April. Still there were two groups of spots—Groups 6181 and 6184—both sufficiently large to be seen by the naked eye, and a third great group—Group 6185—offered a striking example of the rapid development of an outburst. These three groups were all seen early in the month, and had passed off by May 16. From that date to the end of the month, though the Sun was never free from spots, there was little to call for special notice.

Rotation 717 began May 1^{d}·214, and Rotation 718 May 28^{d}·434.

Spot-Groups in 1907 May.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1907.	1907.	days.	d	°	°	
6181	May 2	May 10	9	May 4·2	321	− 9	Fine stream.
2	3	10	8	5·2	307	+15	Straggling stream.
3	4	6	2	2·1	348	+19	Intermittent. Small.
4	4	15	12	9·9	245	−14	**Return.** Large spot.
5	6	15	10	9·9	245	+ 4	Fine irregular stream.
6	8	9	2	6·2	294	+11	Very small spot.
7	11	20	10	15·1	177	−13	Unstable stream.
8	14	17	4	20·1	110	+ 9	Few small spots.
9	16	24	9	22·3	81	+ 8	**Return.** Regular spot.
6190	19	29	11	25·0	46	− 7	Small irregular stream.
1	20	26	7	24·2	56	+ 3	Small spot.
2	21	22	2	20·5	105	+18	Very small spot.
3	22	25	4	18·5	131	−14	Pair of small spots.
4	23	24	2	19·4	119	−22	Small spot.
5	25	30	0	31·4	321	+12	Small regular spot.
6	27	27	1	24·0	59	−16	Very small spot.
7	29	June 9	12	June 4·2	271	−16	Regular spot.

Notes on the Principal Spot-Groups.

Group 6181.—A fine stream arising near the centre of the disc and developing rapidly. Area on May 6 about 750.

Group 6184.—Return of Group 6168. A large circular spot, of area on May 9 about 600. A very interesting development showed itself on May 7 and 8, a bright semicircular rampart forming in the photosphere in advance of the spot, with a semicircular chain of spots, like a moat, in advance of the rampart.

Group 6185.—A fine irregular stream, rapidly developing in size. Area on May 11 about 550.

Group 6189.—Return of Group 6172. A regular spot. Area on May 19 about 60.

Group 6197.—A large regular spot. Area on June 4 about 150.

E. Walter Maunder.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

James Ferguson.

Gentlemen,—

This name will probably suggest to most readers the self-educated Scotchman whose 'Astronomy explained upon Sir Isaac Newton's Principles,' which first appeared in 1756 *, was the first book on astronomy studied by Sir William Herschel, and doubtless by many others of far inferior fame. There were, in fact, few other popular works on the subject until the appearance of Sir John Herschel's treatise in Lardner's 'Cabinet Cyclopædia,' which appeared in 1833, and was long afterwards enlarged into the famous 'Outlines.' Ferguson did much by his lectures to diffuse a knowledge of the science in his day; he was elected a Fellow of the Royal Society in 1763, and died in 1776, at the age of sixty-six.

It is not of him that I wish to speak now, but of a later astronomer of the same name, also a Scotchman by birth, though he early became domiciled in the United States. This James Ferguson was born in the county of Perth on the 31st of August, 1797, and taken to New York in 1800, when only three years old. From the age of 17 to 19 he was employed as assistant engineer in the excavation of the Erie Canal. Later he became an astronomical surveyor appointed by the Boundary Commission nominated to carry out the provisions of the Treaty of Ghent.

It was not until 1842 that the American Congress decided to establish a national observatory at Washington, which was erected under the direction of Captain Gilliss. When it was got into working order in 1845, Lieut. Maury was appointed its Superintendent, and held the office until 1861, but then, on the outbreak of the Civil War, he deemed his allegiance due to the Confederacy, and broke off his connection with the United States Government. On this, Gilliss succeeded him at the Observatory; his sudden death, in February 1865, was the greatest loss, says Prof. Newcomb*, which that institution had ever sustained. When Ferguson first became connected with the Observatory we are not told, but it was in very early times, for Prof. Newcomb states (p. 111 of the same work) that in 1866 he had been the longest in service of any of its force, so that his was one of the names suggested as the head when Admiral Davis, in the second year of his work there, was ordered to sea. But it was decided to have an administrative head of the establishment, on the understanding that he should not interfere with the work of the astronomers. Ferguson, however, died in the following year, on the 26th of September, 1867.

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* Sir David Brewster published a new edition of this work in two volumes in 1821, containing twelve supplementary chapters on recent discoveries.

He was the first American astronomer to discover a small planet by detecting No. 31 on the 1st of September, 1854. It was the next discovery of the kind after Hind's last, which was that of Urania on the 22nd of July in that year, and Ferguson came upon it whilst searching for Egeria, which it was then very near. He soon afterwards named it Euphrosyne, whether from being in a particularly cheerful frame of mind at the time we are not told. The planet has this year enjoyed the singular distinction of being three times announced as a photographic discovery—by Prof. Lowell on the 9th of April, by Mr. Metcalf on the 17th of the same month, and by M. Coggia on the 3rd of May—the identity being pointed out by Prof. Millosevich in *Ast. Nach.* No. 4193.

Ferguson discovered another small planet, now reckoned as No. 50, on the 4th of October, 1857, anticipating Dr. R. Luther, of Bilk, who independently discovered it on the 19th of the same month. The reason why Ferguson selected the name Virginia for it is obvious†. He was a very diligent observer of small planets and comets, no fewer than eighty-four papers being credited to him in the Royal Society's list. He also contributed articles to scientific magazines. His last observations (published in *Ast. Nach.* No. 1680) were of Freia and Undina on the 6th of August, 1867, and he died on the 26th of the following month, soon after completing the seventieth year of his age.

Yours faithfully,

Blackheath, 1907, Aug. 9. W. T. LYNN.

P.S. (*Aug.* 22).—A memorial has recently been erected to the former James Ferguson at Rothiemay, in Banffshire, his native county.

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* 'Reminiscences of an Astronomer,' p. 108.

† One cannot say the same for the Homeric heroes lately placed near the orbit of Jupiter.

The Eclipse of 1030.

GENTLEMEN,—

In vol. xviii. of your Journal (p. 363) you inserted a letter of mine on the eclipse of August 31, 1030, in which I pointed out that it cannot have occurred during the battle of Stiklestad, in which King Olaf lost his life, since the battle must have taken place on July 29. In his paper on this eclipse in the 'Monthly Notices' for last December, Mr. Cowell says that my arguments do not appear conclusive. As the eclipse can only be used for correcting the lunar tables if we may safely assume that it was central at Stiklestad, I hope you will allow me to discuss the question of the date of the battle once more. I have delayed doing so till I had an opportunity of seeing the arguments of Munch ('Det norske Folks Historie,' vol. i. part 2, p. 787 *sq.*), as he is the only historian who follows Hansteen in accepting Aug. 31 as the date of Olaf's death.

Munch has, of course, to acknowledge that the sagas and chronicles all give the date as Wednesday, July 29, with the one exception of the 'Lübeck Passionale,' which has "a Friday in August 1028." This he suggests is a misunderstanding of "feria quarta, quarto Kal. Aug.," the words "quarto Kal." being overlooked. He enters into an elaborate calculation as to how long it may have taken Olaf to hear of Earl Haakon's death and to reach Stiklestad from Russia, and he decides that Olaf can hardly have arrived at Stiklestad before the end of August. But as we do not know when Haakon died, except that he was lost at sea soon after St. Martin's Day, November 11, 1029, it seems risky to make out that Olaf cannot have arrived about the end of July. Hansteen lays special stress on the statement (in St. Olaf's Saga and by Snorre) that the king's body was found by one of his adherents the night after the battle, "when it had become dark," and that the same man the following night, in the presence of people, sunk a coffin filled with stones in the fjord, and then rowed past Trondhjem, when "it was now quite dark," and secretly buried it. It is also said that the peasants lay in wait for a fugitive after the battle in order that he should not escape in the night, "for it was not so light that one could continue the fight." Hansteen remarks that about the 29th July the Sun would at midnight only be 8° 39′ below the horizon, so that there would be no darkness at all, while at the end of August it would be dark for six hours. This is, of course, indisputable; but if we accept every word of the accounts as literally true, I am afraid we ought also to accept all the miracles which, according to the same accounts, happened during the night, for why should the account of the darkness be more trustworthy than that of the miracles? Perhaps the darkness which hid the burial of the king from his enemies was as supernatural as that which occurred when he was slain.

Within a year of his death Olaf was recognized as a martyr and saint even by the very men who had fought against and slain him.

His body was exhumed on the 25th July, 1031, and re-buried for nine days to see if it would continue to rise towards the surface, which of course it did. On the 3rd August he was solemnly proclaimed a saint, and within ten years he was venerated as such all over the north of Europe[*]. Is it likely that any mistake should have been made as regards the date of his death? He did not die in an obscure place; he was killed in a battle which must have been talked about a good deal, and the 29th July was at once fixed on as his day. Munch mentions that one source, though it says he was killed on July 29, calls this day his *natalitium*, and he thinks that this shows that some people must have remembered that this day was really Olaf's (supposed) birthday, and not the day of his death. But Munch has overlooked the fact that the writer meant Olaf's birthday in heaven, and used an expression frequently employed when speaking of martyrs. For instance, in the 'Acta Sanctorum' (Julii, t. vii. p. 118 F) the day is spoken of as Olaf's *dies natalis*, which from the context can only mean the day he was born in heaven. Hansteen suggests that the Pope had to fix a different day for Olaf's festival because August 31 was already occupied by a German saint, Paul of Treves, whose priority could not be set aside. But unfortunately the Pope had nothing to do with the matter, for Olaf was never formally canonized[†]; he was only made a saint by popular acclamation, and it is not likely that the people of Norway would have cared whether the day already belonged to a German saint of no particular renown, especially as most days in the year belong to several saints.

In my letter of 1895 I mentioned that the desire of magnifying the saintly character of Olaf must have caused the eclipse which happened a month later to be referred to the day of the battle, the chroniclers wishing to make out that the darkness said to have occurred at the death of Christ also happened at the death of Olaf. In fact the older Olaf's Saga expressly compares the two events, while, on the other hand, neither the 'Fagrskinna Saga'[‡] nor Theodoric the historian (Tjodrek Munk) mention the darkness during the battle. I am still of opinion that there is no way of getting over the fact that the 29th July has been St. Olaf's Day from the year after his death, and that the sagas and chronicles say that he was killed on that day. Therefore we have no contemporary testimony as to where the line of central eclipse crossed Norway.

Yours faithfully,

J. L. E. Dreyer.

Armagh Observatory,
1907, Aug. 20.

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[*] Before 1040 Duncan, Norwegian Bishop of Dublin, gave to Christ Church Cathedral pieces of Olaf's clothes as relics, and he is mentioned as a saint in English chronicles about 1046.

[†] The only Papal confirmation he ever got was nearly 600 years after his death, when his worship in Poland was sanctioned by apostolic authority (Acta Sanct. *l. c.*).

[‡] This Saga was written a few years before Snorre wrote (Storm, Snorre Sturlassöns Historieskrivning, p. 44).

Gentlemen,—

The astronomical statements of the narrative are essentially three, (α) that there was a total solar eclipse, (β) that a battle was fought on the same day, (γ) that there was totality at Stiklestad. Dr. Dreyer argues against (β); in estimating the credibility of (γ) we must no doubt give due weight to Dr. Dreyer's arguments against the credibility of (β), but we must also remember that (α) is confirmed by modern astronomy and that (γ) is not directly challenged. Under these circumstances I think I was justified in making the calculations necessary to compare statement (γ) with similar statements relating to other eclipses. I do not attach any great importance to this eclipse, but it is certainly satisfactory to me to find that it is consistent with my theory. I rely, not on single eclipses, but on the concurrence of several. The eclipse of Thucydides and the eclipse of Babylon cast grave doubt upon the accuracy of the present tables, but all other eclipses I should regard as worthless if taken singly. Yours faithfully,

P. H. Cowell.

Bradley and the Sun's Motion in Space.

Gentlemen,—

In his Presidential Address to the British Association, Sir David Gill says: "Tobias Mayer, in 1760, seems to have been the first to recognize that if our Sun, like other stars, has motion in space, that motion must produce apparent motion amongst the surrounding stars." In so saying he overlooks the fact that, at least 12 years before, Bradley had perceived the same thing, and had pointed it out in the concluding paragraphs of the communication printed in the 'Philosophical Transactions' for 1748, in which he announced the discovery of Nutation. Grant, in his 'History' (p. 554), briefly alludes to the passage, and Humboldt gives part of it in 'Cosmos,' but it seems but little known. Bradley has been speaking of some of the details of his research, and goes on:—"The fore-mentioned points, indeed, can be settled only on the supposition that the angular distances of these stars do continue always the same, or that they have no real motion in themselves, but are at rest in absolute space. A supposition which, though usually made by astronomers, nevertheless seems to be founded on too uncertain principles to be admitted in all cases." He then mentions the probability that some stars really do exhibit changes not parallactic in their relative positions, instancing, as "a strong proof of this," the fact that the differences between the then declination of Arcturus and that derivable from the observations of Tycho or of Flamsteed were too great to be attributable to errors in their observations, and then proceeds:—"It is reasonable to expect that other instances of the like kind must also occur among the great number of the visible stars; because their relative positions may be altered by various means.

For if our own solar system be conceived to change its place with respect to absolute space, this might, in process of time, occasion an apparent change in the angular distances of the fixed stars, and in such a case the places of the nearest stars being more affected than of those that are very remote, their relative positions might seem to alter, though the stars themselves were really immovable. And on the other hand, if our own system be at rest, and any of the stars really in motion, this might likewise vary their apparent positions; and the more so the nearer they are to us, or the swifter their motions are, or the more proper the direction of the motion is, to be rendered perceptible by us. Since, then, the relative places of the stars may be changed from such a variety of causes, considering that amazing distance at which it is certain some of them are placed, it may require the observations of many ages to determine the laws of the apparent changes, even of a single star; much more difficult, therefore, must it be to settle the laws relating to all the most remarkable stars." He then, assuming that the brightest stars are probably the nearest, goes on to point out the method of investigating their changes of position, parallactic or other, by comparing their places with those of small stars close by. (The version of the passage in 'Cosmos' is evidently only a re-translation of a translation of the original.)

Two years later Thomas Wright, of Durham, published his 'Theory of the Universe,' of which the doctrine that all the stars, including the Sun, were in motion in space, formed part; but as it had been long growing up, this idea may have been independent of Bradley's words just quoted. As to Mayer, Grant states that his conclusion, based upon an investigation of 80 stars, was "that the observed proper motions of the stars did not afford evidence of the solar system being transported through space to any particular region of the heavens."

Yours truly,

Glasgow, 1907, Aug. 14. S. MAITLAND BAIRD GEMMILL.

The Maintenance of the Sun's Heat.

GENTLEMEN,—

The idea involved in the following remarks on the maintenance of the Sun's heat may be new to some of your readers. What the Sun does is to set the ether in vibration. When the vibrations which owe their origin to the Sun reach the Earth or any other material body, they are interrupted and, in fact, converted into small motions among the atoms of the material body —which small motions constitute light and heat. But supposing that the vibrations in the ether never encountered any material body, what would become of them? Is it not at least possible that the ether once set in vibration would always continue to vibrate, and that therefore the continuance of these vibrations would involve no expenditure of energy on the part of the Sun or other body which caused them? The ether would thus act merely as a medium between material bodies, conveying to one the vibratory motions set up by another to be converted (or re-converted) into heat and light on reaching their destination. It conveys heat from every material body to every other material body in the Universe, but it acts only as a medium, and never absorbs or emits heat or light itself. The quantity of heat in the Universe is neither increased nor diminished by this medium, which conveys heat from one place to another, but loses none on the way. Indeed if any heat were lost in transmission the law of inverse square would become inapplicable.

To a spectator at the centre of the Sun the Earth would only cover 1-23,000,000,000th part of the sky. It is therefore often assumed, that if we can determine the amount of heat which the Earth receives from the Sun per minute, we can find the total amount which it emits per minute by multiplying this amount by 23,000,000,000. This supposes that the solar energy is equally expended in directions where there is nothing to interfere with the vibrations, and in directions where they encounter a solid body which absorbs them—in short, that when the ether-particles are set in vibration they would instantaneously return to their former position of equilibrium unless a new force were applied to restore them to the same vibratory condition. Why may not their vibrations resemble the swing of a pendulum, which would never cease to swing backwards and forwards if we could exclude all causes that tend to arrest its motion? Very possibly, as seen from the centre of the Sun, not more than 1-23,000th part of the sky would be covered by any kind of material substance; in which case, according to my hypothesis, the total amount of heat expended by the Sun per minute would be only one million times what it expends on the Earth, or 1-23,000th part of what it is usually supposed to be expending.

That the Sun is losing heat I think probable in any event, for its near neighbours are dark bodies, while all the bright ones are very remote. The exchanges of heat are therefore probably unfavourable to the Sun. But this loss of heat would probably be too slow to have produced sensible effects within any moderate period, even if shrinkage were altogether out of the question. And it is difficult to believe that the Sun (and the same thing might be said of all other bright stars) is constantly parting with its heat to the ether, *which neither absorbs that heat nor gives it up to any material body*. But is not this the result of what I may call the current theory?

Truly yours,

W. H. S. MONCK.

Dublin, 1907, Aug. 12.

PUBLICATIONS.

'ASTRONOMICAL ESSAYS, HISTORICAL AND DESCRIPTIVE' *.—We have in this volume a collection of essays on a great variety of subjects. The book begins with a chapter on primitive astronomy and the origin of the constellations. The author then deals with the old Greek astronomers, the names of the stars of Arabic origin, the evolution of astronomical instruments, and modern theories. An interesting account is given of the astronomical work of the Rev. John Michell. A long chapter is devoted to Sir William Herschel's theories and observations. The descriptive essays are largely on Stellar Astronomy. The size, brightness, and number of the stars, and the dimensions and form of the stellar universe, are more particularly dealt with. Among the miscellaneous essays we note one on the "Ringed Planet," and another on "A possible Celestial Catastrophe." The book concludes with an account of the most recent development of the Nebular Hypothesis.

In his account of the constellations the author expresses the opinion that the "formation of the constellations was not completed before the time of Eudoxus." As examples of constellations of late formation, Virgo, Libra, and Coma Berenices are given. We think it unlikely that two of the zodiacal constellations were formed after the other ten, although the *names* may be of recent origin. With respect to Coma we suspect the only thing done by Conon was to append the honorary designation "Berenices"—in fact we see no reason for modern astronomers continuing to give this one constellation court rank. Berenikê might very well be buried in quiet oblivion along with Sobieski and Poniatowski. Mr. Gore tells again the story of the Chinese astronomers of the 22nd century B.C., Hi and Ho, who were executed "because they failed to announce a great eclipse of the Sun which occurred about that time." Unfortunate Hi and Ho! They were born before their time. Imagine (if you can) Halley's Comet coming back unexpectedly, and, as a consequence, the modern representatives of Hi and Ho being suspended from the yard-arm of the ship on the top of the dome of the Greenwich equatorial!

The most interesting of the historical chapters is the translation of Ptolemy's account of the Milky Way. Every schoolboy knows Ptolemy by name, but how many astronomers have ever seen his great astronomical work, much less read it? There is no English translation of the 'Almagest,' and we ought therefore to be the more grateful to Mr. Gore for giving the English reader an opportunity of perusing this minute account of the Milky Way written some 18 centuries ago.

In the essay on Sir W. Herschel's Theories we have an account of Herschel's method of estimating the space-penetrating power of a telescope. Suppose the diameter of the pupil of the eye to be represented by a, and the aperture of the telescope by A, then the penetrating power of a telescope is represented by $\sqrt{x}A/a$, where x is a coefficient dependent on the loss of light by reflection from mirrors and by absorption of lenses. According to this, since x is always less than 1, if $A=a$, the space-penetrating power would be somewhat less than that of the unaided eye. Now, as Herschel estimated the diameter of the pupil of the eye to be 0·2 inch, it would follow that a telescope of 0·2-inch aperture would not show the faintest stars visible to the naked eye. It seems very remarkable that a practical astronomer of Sir William Herschel's vast experience and great ability should not have thought of stopping down a small telescope to the aperture of the eye, and ascertaining by actual observation what the penetrating power of such a telescope was in reality. If he had done so, he would have discovered that his formula only gave about half the actual value of the space-penetrating power. The actual result would have depended on the focal length and power of the telescope used for the experiment. With one particular telescope we found that any star visible to the naked eye could be seen through the telescope when the aperture was stopped down to $\frac{1}{8}$th inch. The reason is obvious. The visibility of a star depends on the contrast between the light of the star and the dark background of the sky. In a telescope the effect of the magnification of the field is to reduce the light of the background while it leaves the light of the star unaffected. This is the reason we can see stars with a telescope by daylight. The diminution of the faint luminosity of the field of view in a telescope with the increase of the power of the eyepiece seems to us a fact which would force itself on the attention of anyone who was in the constant habit of using a telescope. Yet it does not appear to have been noticed by Herschel. For he thought "that the light of the heavens on a clear night would set a limit to the faintness of the smallest stars visible" (p. 169). On the contrary, we have the means of practically eliminating the light of the sky by increasing the magnifying-power used. This remark, of course, does not apply to nebulæ or any object having a sensible angular diameter.

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* By J. Ellard Gore, F.R.A.S. London: Chatto & Windus, 1907.

A similar remark applies to celestial photography. In photographing a nebula the time of exposure required depends on the quotient f/a, just as in ordinary photography. In photographing a star the time of exposure depends on the linear aperture of the lens. We thus have it in our power, by using a lens of long focus, to diminish the general darkening of the plate due to the diffused light of the sky. We do not think there is sufficient ground for the opinion "that stars of the 18th magnitude are probably the faintest which could be photographed" (p. 169).

In looking over those pages in which the Stellar Universe is the subject-matter, we are struck with the great change which has come over the opinions of astronomers with respect to the dimensions of the system of the stars. We believe, a generation ago, the prevailing idea was that the magnitude of a star was mainly determined by its distance, and that, as we increased the power of our telescopes, so the number of stars rendered visible would continually increase, the only limit being the practical one of constructing a large telescope. At the present time the current belief appears to be that the Stellar Universe is a gigantic cluster of stars, limited in number, and occupying, comparatively speaking, a space of moderate extent. Mr. Gore quotes the observations made by the late Dr. Roberts to the effect that plates exposed during 10 or 12 hours to the sky only depict the same faint stars as are seen upon plates exposed for 1½ hours. The number of visible stars is far short of 1000 millions—probably does not exceed 100 millions (p. 225). This large number is comparatively small. It is less than the number of grains of corn in a ten-acre field of ripe oats. The total number of stars in the universe is probably much less than the number of human beings living on our little globe (p. 280). It may be objected that all this only applies to luminous stars; the number of dark bodies in space may be enormously greater, or infinite. Against this we may put the opinion of Lord Kelvin, who says that purely dynamical considerations "lead to a decisive proof that the mean density of ponderable matter through any very large spherical volume is smaller the greater the radius." In other words, ponderable matter thins out in just the same way as the visible stars. Again, "it seems perfectly certain that there cannot be within this distance [1000 stellar units] as much matter as 10,000 million suns" (Baltimore Lectures, not referred to by the author).

In Chapter XXI. we have a discussion of the probable distance of the more distant stars in the Milky Way. Different estimates are given varying from 3000 to 9680 "light-years." We regret that the author adopts the unscientific "light-year" as a unit of distance. It seems to us that decidedly the most convenient unit for the purpose is that which makes the number of units the reciprocal of the parallax.

In the Essay on "Holes in the Heavens," the author calls attention to those remarkable dark regions sometimes called "coal-sacks." They may be caused in two possible ways: they may be due to masses of cooled-down matter which absorb the light of the stars behind them, or they may be really openings through regions of stars or nebulous matter. Mr. Gore adopts the latter view. In this case the existence of these holes indicates that the stratum of stars in which they occur can only have a small extension in the line of sight. We do not think these vacuities have received the attention they deserve from astronomers. A careful examination of such vacuities should furnish data for a conclusion as to their nature—whether they are dark masses obstructing our vision, or real holes in the stellar system through which we look into empty space.

After perusing this part of the volume there are two questions that arise in our mind which are not touched upon by the author. The first is, If the universe is finite in space, is it finite in time? To this we apprehend the answer to be that, if the universe is finite, it must have had a beginning, and must in time come to an end. The other question is, If the stellar universe is contained in a sphere of, say, 1000 stellar units radius, what is there beyond? To this the astronomer will reply that theories and hypotheses are put forward for the purpose of explaining observed facts; when there are no facts to be explained, no theory is required. As there are no observed facts as to what exists beyond the furthest stars, the mind of the astronomer is a complete blank on the subject. Popular imagination can fill up the blank as it pleases.

The essay on a "Possible Celestial Catastrophe" deals with the possibility of the Sun some day coming into collision with a dark body. Mr. Gore says "the effects of such a collision may be easily imagined." We confess to having experienced some difficulty in exercising the required amount of imagination. We quite admit that "a stupendous amount of heat would be produced," but whether the heat would be sufficient to destroy the Earth and the other planets may be open to doubt. The effect upon the Earth would not depend on the quantity of heat generated, but upon the rate at which the heat was radiated. This, again, would depend upon the surface of the combined Sun (consisting of original Sun and dark body) and upon its surface-temperature. It seems to us that the circumstances of the collision might be such as only to increase the daily amount of heat received by the Earth to a moderate extent. For instance, the Sun might be transformed into a flat disc whose edge might not have an excessively high temperature. If, then, the disc were in the plane of the Earth's orbit, the effect on the Earth might be comparatively trifling. Perhaps there would only result a re-arrangement of climate. The temperate zones would become tropical, and the arctic regions temperate. Only the equatorial parts of the globe would become uninhabitable. Mr. Gore appears to assume (p. 300) that the dark body must come from the neighbourhood of the apex of the Sun's way. Clearly there is no such limitation. With a suitable velocity and direction it might come from any part of the heavens.

In the chapter on the "Brightness of Starlight," the question of the total amount of light received from the stars is discussed. We have here some interesting remarks on the luminosity of the sky. Mr. Gore confirms the fact, which has been noticed by Mr. Denning and Mr. Backhouse, and perhaps by others, that the dark background of the sky seems on some nights comparatively dark, and on others comparatively luminous (p. 220). He also notices the remarkable and unexplained fact that the brightness of the sky increases perceptibly near the horizon. We are inclined to the belief that the luminosity of the sky is largely due to the same cause (whatever it may be) that produces the zodiacal light and the gegenschein.

We have only noticed a few of the matters treated in this volume, which we commend to the consideration of all students of astronomy. G. J. B.

NOTES.

Comet Notes.—*The Periodical Comet* 1881 V. The time of revolution of this comet, as given in Dr. Matthiessen's definitive orbit, was 8·68 years, and three of these intervals, counting from the perihelion passage on 1881 September 13, indicate that a return of the comet should now be expected under the same favourable conditions as those which prevailed in 1881. Dr. David Smart has kindly prepared a sweeping ephemeris on the assumption of perihelion passage on October 6th next, and it may serve as a useful guide to those who may be disposed to search for the object. Dr. Smart has pointed out that the ephemeris is likely to be more erroneous in R.A. than in Declination. Perturbations have not been allowed for, but are not likely to have been very considerable, as the comet has not been near Jupiter since its last observed return.

In 1890 May and 1899 February, when the two intervening visits of this comet since its discovery were made, its position rendered it invisible. The comet is rather a notable one, as at one point of its orbit it approaches to within 3½ million miles of the Earth. At its visit in 1881 August it was calculated to have been visible to the naked eye, but was then traversing the southern constellations. When discovered at Bristol on October 4 at 3 A.M. it had lost much of its brightness. Only 37 observations of the comet were made, and it was last seen by Winnecke, through the great refractor at Strasburg, on November 24.

Ephemeris for Greenwich Midnight.

1907.	R.A. h m	Dec. ° ′	log Δ.	Light.
Sept. 4......	6 44·5	+17 50	9·876	1·58
8......	7 15·1	+17 57	9·885	1·66
12......	7 44·7	+17 50	9·897	1·71
16......	8 13·6	+17 28	9·912	1·74
20......	8 40·8	+16 50	9·930	1·72
24......	9 6·6	+15 59	9·951	1·67
28......	9 30·9	+14 58	9·973	1·58
Oct. 2......	9 53·7	+13 49	9·995	1·47
6......	10 15·3	+12 33	0·017	1·34

W. F. Denning.

Comet *d* 1907 (Daniel) is now (Aug. 13) a conspicuous object in the east in the early morning, the nucleus being certainly brighter than a star of the second magnitude.

Photographs showing the tail extending to about 100 minutes of arc from the nucleus (*i. e.* to the edge of the field photographed) were obtained at Greenwich by Mr. Melotte with the 30-inch reflector on August 10, 11, 12, and 13. On these photographs the tail is shown to consist of some 15 to 20 streamers, and its structure appears very complicated.

Small-scale photographs were also taken with the R.R. (Dallmeyer) lens on August 11, 12, and 13. The greatest extension photographed is on the plate taken August 13, on which the tail can be traced to 7 degrees from the head of the comet.

The general direction of the tail is west, and the streamers gradually spread out, fan-like, and curve to the south.

Ast. Nach. No. 4194 contains elements of the comet by George Dybeck from observations on June 14, 27, and July 11; the middle place is represented within 1″ in longitude, 8″ in latitude, showing that there is no great departure from a parabola:—

T	1907 Sept. 4·0239 Berlin M.T.
ω	294° 14′ 18″
☊	143 0 51
i	8 57 22
log *q*......	9·71131

The comet is becoming rapidly fainter, and rises about an hour before the Sun in the middle of September. The following ephemeris is for Berlin midnight:—

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Sept. 4....	9 10 27	12 41	Sept. 10....	9 27 26	10 46
6....	9 23 8	12 4	12....	9 59 1	10 6
8....	9 35 28	11 25	14....	10 10 13	9 26

Astr. Journ. 596 contains definitive elements of Comets 1796 (Olbers) and 1822 III. (Pons) by Henry A. Peck; they are referred to the equinoxes of 1796, 1822. The first comet was only seen for a fortnight, and the elements are somewhat uncertain. The second one was seen for a slightly longer interval, and although many of the observations were only made with a sextant, and are rough, the fact that it passed very near the Earth (less than 0·14) renders the elements fairly certain:—

T....	1796 Apr. 2·7737 G.M.T.	1822 July 15·8337 G.M.T.
ω....	184° 17′ 5″	237° 44′ 16″
☊ ..	17 2 40	97 43 32
i	115 8 50	143 41 50
log q..	0·19856	9·92795

Dr. Wolf has found an image of Comet 1905 IV. on a plate taken 1904 Jan. 10, 783 days before discovery. Its mag. was 15½. Thus the observations already cover an interval of 3½ years, and it is quite likely that this period will be further extended.

A. C. D. C.

MINOR PLANET NOTES.—New planets, UN^a, UN^b, were discovered by Wolf at Heidelberg, 1906 July 16 (mags. 14, 13½). Another, ZT, was discovered there by Lohnert, 1907 July 19 (mag. 11½). Planet ZB proves to be identical with 31 Euphrosyne; an error in the 'Berliner Jahrbuch' prevented the identity from being detected sooner.

Ast. Nach. 4193 contains new elements of the planet VY Patroclus, belonging to the Jupiter group:—

Epoch	1906 Nov. 29·0 Berlin M.T.
M	41° 27′ 30″
ω	302 11 27
☊	43 25 32
i	22 3 33
ϕ	8 16 7
μ	300″·659
log a	0·714621

Planet WE has been named Drakonia. A. C. D. C.

MARTIAN PHOTOGRAPHY.—Prof. Lowell and his assistants have repeated their success of 1905 in photographing the Martian canals. Prof. Lowell has forwarded a print of a plate taken by him on July 11, showing 22 canals, and also enabling the outlines of the bright and dark regions to be accurately measured.

A. C. D. C.

IN the July No. of the *Astrophysical Journal* Prof. Newcomb has an interesting paper dealing with the Martian Canal System. He remarks that the principles involved are of two classes—one optical, which includes all causes affecting the formation of an image on the retina, the other psychological, embracing all causes which affect the observer's perception of this image. The principal agencies in the first class are the atmosphere, the instrument, and the eye.

He then proves that an angular geometric breadth of from 0″·01 to 0″·02, representing 3 or 4 miles on the surface of Mars, will spread out to a dark band of 0″·2, with a shading from 10 to 20 times as broad. So that in linear measure on the planet the band will be upwards of 40 miles in breadth instead of only 3 or 4 miles. This necessarily diminishes its visibility.

He finally concludes that the actual breadth of the narrowest visible canals on Mars must exceed 10 miles, and may be as great as 20 miles. Adding the border of 20 miles on each side, necessarily produced by aberration, diffraction, and softening, the apparent breadth in the telescope and on the retina would be 50 miles and upwards. Making due allowance for the aberration of the best achromatic telescope, the total area of the 400 canals can scarcely fall much below one-half the whole surface of Mars, and may be greater. These results do not disprove although they weaken the objective reality of the canal system.

The psychological aspect of the question is also dealt with, and illustrations of experiments are given which show how wide an interpretation can be given to the observations.

SPIDER-WEBS.—A paper in the *American Journal of Science* for June is of some astronomical interest. Dr. Benton found a spider-thread 2·5 m. long and 0·01 cm. in diameter, and was thereupon induced to make some experiments. A spider-thread, as is well known, is not usually composed of a single fibre and in the thread under examination the number of components was very large, probably several hundreds. The experiments showed that the material possesses a high tensile strength, about double that of most kinds of wood. The length of the thread varied irregularly from day to day the stretching-force being constant, and the elongation at rupture amounted to about 20 per cent. of the original length of the thread.

THE usual watch for the Perseid Meteors was kept at Greenwich from August 10 to 13. Very few brilliant meteors were seen, and on the whole the display was a very poor one, the actual number of meteors bright enough to be plotted being 27 on August 10–11, 64 on August 11–12, and 8 on August 12–13. Clouds frequently interrupted observation on the last night, but on the other occasions the sky was quite cloudless.

SUCCESSFUL photographs of Phœbe, the ninth satellite of Saturn, have been obtained at Greenwich by Mr. Melotte with the 30-inch reflector on several dates. Provisional measures of position-angle and distance show that the ephemeris published by Dr. Ross in the American Ephemeris, 1907 (second edition), is essentially correct.

ERRATUM.—In August number, p. 322, line 31, *for* "snall" *read* "small."

WE are pleased to announce that the first batch of reproductions of Chart plates taken at Tacubaya Observatory have been received, the prints being similar to those issued by the French observatories. *These 23 plates* selected for reproduction have their centres at

Declination 16° South and between Right Ascension $0^h\ 0^m$ and $5^h\ 44^m$.

It has been arranged that Mr. H. C. Plummer, of the Oxford University Observatory, should hold the position of Fellow in the Lick Observatory for one year from September. He sailed from Liverpool on August 17. This we believe is the outcome of a desire of Mr. Plummer to make himself thoroughly conversant with spectroscopic work.

We are sorry to announce the death, on August 13, of Dr. H. C. Vogel, Director of the Astrophysical Observatory, Potsdam.

FROM AN OXFORD NOTE-BOOK.

The Leicester Meeting of the British Association will live in my memory as an occasion when I had the honour of sitting at table with the Astronomical Society of the Atlantic. And the occasion was highly appropriate. Not only was the visit to South Africa which gave the Society birth still fresh in remembrance, but the eminent astronomer who made the visit itself a possibility was now President of the Association, and as such played the double role of Guest of the Evening and Member of the Society. Moreover, his Presidential Address paid a well-deserved tribute to the work of an absent Member of the Society. The unavoidable absence of one or two Members was naturally much regretted, but the remainder was well able to maintain the high reputation of the Society. Their notion of the constitution of a dinner will bear comparison for intelligence with that of any other Society I have as yet had the good fortune to meet; and if only one could remember, and feel at liberty to repeat, all the stories! A stray sentence or two lingers on the mental retina:—" And he was a member of the Anglican Church, and had a great argument with a Jesuit, which lasted three whole days; and at the end of that time he became a Jesuit, and the Jesuit joined the Anglican Church."

Section A of the British Association is making heroic struggles to regard the transmutation of metals, and earthquakes, and magic squares, and secular stability, and weather forecasting, and the variation of latitude as the same branch of Science under different aspects. There are some difficulties in the way, and various suggestions are forthcoming for meeting them. Sir Oliver Lodge thinks (see this week's *Nature*) that the Section must refrain from excursions on Saturday, and sit for six days instead of four in order to glue all these things together; but it seems doubtful whether such a plan is feasible in these days of many other meetings. The time spent is, in a large number of instances, taken out of holiday, and a continuous week listening to papers in a room often poorly ventilated, without the break of a Saturday excursion, is not a very first-rate holiday. It would almost seem as though some sort of subdivision of the Section were the lesser evil.

PROF. LOVE's address to Section A is well worth reading. Following up the clue of gravitational instability, due to Prof. Jeans, he has shown the possibility of explaining the approximate shape of our continents on simple assumptions. Prof. Jeans considered only one of these assumptions—a displacement of the Earth's centre of gravity to one side of the centre of figure, represented mathematically by a spherical harmonic of order 1. Prof. Love has added the harmonics of orders 2 and 3: a possible origin of the order 2 being the Moon's attraction long ago, when she was "fixed in the sky," and may have permanently elongated the Earth; and order 3 being chiefly due (if I understand Prof. Love rightly) to a combination of 1 and 2. It is one of the prettiest ideas in his address that 3, the child of 1 and 2, and originally much smaller than either, has now become comparable with them in size, not by its own growth, but by the dwindling of its parents from causes which leave itself unaffected. It will be at once obvious how suggestive are such ideas in all matters relating to the past history of the Earth; and Prof. Love was fortunate enough to be able to illustrate this far-reaching influence by comparison with an early and sagacious inference of Charles Darwin. It was an enjoyable moment for us all when Charles Darwin's son rose to congratulate the reader of the address, and to admire, with the admiration born of experience in neighbouring fields, the simplicity of the apparatus which had been found sufficient.

WHEN a man, however careful and skilful, announces the transmutation of metals as an accomplished experience, I suppose people are puzzled how to take it. Sir William Ramsay says he has obtained various "elements," including lithium, from other "elements," and the audience did not cheer but stared. I suppose we dare not make a mistake, even as an audience? and yet I think we shall be sorry, if this proves to be the first step in a new chemistry, that we let it pass in silence, because we feared to take, as mere recipients of the news, one-thousandth part of the risk the experimenter took in offering it to us.

WHAT's in a name? The *Times* of July 24 records an action to restrain a well-known comedian from producing a dramatic sketch called "Astronomy," which the plaintiff alleged was a colourable imitation of his sketch called "Motoring, or the Motorist":—

> The learned Judge, on the facts, held that, although there was a great deal of dissimilarity between the two entertainments, there was a substantial

similarity; the principal actor in one piece imitated the principal actor in the other; and therefore a great deal of the defendant's entertainment would have to be restrained.

Had this taken place in California, within knowledge of Prof. Hale's "automobile" which carries giant telescopes up Mount Wilson, the easy transition from Astronomy to Motoring might be more readily understood. But perhaps the reputation of this wonderful vehicle has spread more widely than we thought?

HERE is another item of interest from the *Times* (August 19):—

A MOORISH ASTROLABE.—Mr. G. L. Frolick writes from 1 Arundel Street, Strand, to the effect that a Stockholm newspaper, the *Aftonbladet*, gives a detailed description of a remarkable Moorish astrolabe discovered and brought to Sweden by the well-known traveller, Major Wester. The date is 1329, and it is claimed that the instrument is more complete than any of the four existing specimens, particularly in having a planisphere "for all latitudes." Major Wester has also acquired a curious Moorish sextant. Both these instruments will be shown before the Swedish Academy of Sciences by Professor Bohlin, chief of the Stockholm Observatory. The prices paid at Fez were 150f. for the astrolabe and 120f. for the sextant, and the finder has already received an offer of £4500 for the former.

THE following sentence is from a sermon delivered in Ditton Parish Church on April 21, and has been kindly sent by two correspondents, one of whom adds that the "preacher was not the Rector":—

I wonder if you can think of the Sun except with reference to the Earth. I fancy it would be difficult to imagine it rising morning after morning and pouring out its light and heat into limitless space.

FROM a paper on the Zenith Telescope:—

All openings are kept closed during the progress of the observations.

THERE will apparently be some good reading in 'The Letters of Wotton,' edited by L. Pearsall-Smith, shortly to be published by the Clarendon Press. An advertisement gives several extracts of much promise; one about Kepler's camera obscura, in a letter to Bacon dated 1620, ending thus:—

This I have described to your Lordship because I think there might be good use made of it for chorography; for otherwise, to make landscapes by it were illiberal, though surely no painter can do them so precisely.

But no apology is needed for quoting the following in full. It is from a letter to the Earl of Salisbury, dated March 13, 1610, and they were apparently not too squeamish in those days to show a little excitement over scientific discoveries while they were still fresh:—

I send herewith unto his Majesty the strangest piece of news (as I may justly call it) that he hath ever yet received from any part of the world; which is the annexed book [Galileo's *Sidereus Nuncius*] (come abroad this very day) of the Mathematical Professor at Padua, who by the help of an optical instrument (which both enlargeth and approximateth the object) invented first in Flanders, and bettered by himself, hath discovered four new planets rolling about the sphere of Jupiter, besides many other unknown fixed stars; likewise, the true cause of the *Via Lactea*, so long searched; and lastly, that the Moon is not spherical, but endued with many prominences, and, which is of all the strangest, illuminated with the solar light by reflection from the body of the Earth, as he seemeth to say. So as upon the whole subject he hath first overthrown all former astronomy—for we must have a new sphere to save the appearances—and next all astrology. For the virtue of these new planets must needs vary the judicial part, and why may there not yet be more? These things I have been bold thus to discourse unto your Lordship, whereof here all corners are full. And the author runneth a fortune to be either exceeding famous or exceeding ridiculous. By the next ship your Lordship shall receive from me one of the above-named instruments, as it is bettered by this man.

WE all owe sincere thanks to the Belgian Astronomers for the compendium of observatories and astronomers just issued ('Les Observatoires Astronomiques et les Astronomes,' par P. Stroobant, J. Delvosal, H. Philippot, E. Delporte, et E. Merlin. Published by Hayez, of Brussels, Rue de Louvain 112). No price is marked *, but one feels inclined to say "Whatever it is, it is well worth it." The want of such a general list must have been widely felt since the handy little volume compiled by the late Prof. Lancaster went out of print—and that is many years ago. It is to be hoped that some plan of making additions and corrections conveniently and frequently may be devised now that a new start has been made. There is a map at the end showing the distribution of observatories, and bringing out in a striking way the injustice done to the southern hemisphere.

PROF. SCHAEBERLE announces in *Science* for July 26 a novel view of the Universe:—

> For some years past I have been engaged in observational and theoretical work on that intricate problem—where is the origin and what is the physical structure of our sidereal system? The results so far obtained are novel, since they indicate that the structure is radial, in other words the stars and nebulas of our system are moving either directly towards or directly away from our Sun; the observed deviations from radial motion being attributed to the unsymmetrical distribution of the attracting masses, and also to the presence of bodies having a secondary origin.
>
> The indications also point to the conclusion that, as seen from our Sun, a vast majority of the stars and nebulas are confined to a region whose radial depth is much less than the distance of this region from our Sun. Since bodies so situated may be comparatively near to us and still have various radial velocities without causing sensible changes in the configuration of the heavens, the seemingly unchanging aspect of the Milky Way and other celestial regions is explained without the necessity of assigning such great distances (and consequently such great masses) to the bodies of our system.

His object in making this announcement is to "emphasize the fact that in the present state of our knowledge the approximate distance of any particular star must still be regarded as an unknown quantity." We shall all await the full memoir with interest.

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* Later: the price is 7½ francs.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

Vol. XXX. OCTOBER, 1907. No. 388.

Our Debt to Astronomy *.

Astronomers are so often asked questions such as "What is astronomy good for?" "Why waste your time and energy upon anything so immaterial and unpractical as astronomy?" "What good does it do to know where a comet is going?" &c., that it seems opportune and fitting to answer such questions quite thoroughly. For this reason the subject "Our Debt to Astronomy" has been chosen for this address. These questions and many others of a similar nature show how little thought is given by the general public to such matters, and how carried away they are with the intensely practical ideas of this rapidly advancing age, which seems to have emblazoned upon its banner not "Excelsior," but a glaring monogram of the United States with the curve at the bottom of the U eliminated from the picture. Among other things I hope to convince the followers of this banner of the almightly dollar that astronomy is one of its most potent aids and should therefore be one of its pets, worthy of its support.

Upon this subject Prof. Young writes as follows in the introduction to his 'General Astronomy':—

> At present the end and object of astronomical study is chiefly knowledge pure and simple; so far as now appears, its development has less direct bearing upon material interests of mankind than that of any other of the natural sciences. It is not likely that great inventions and new arts will grow out of its laws and principles, such as are continually arising from physical, chemical, and biological discoveries, though of course it would be rash to say that such outgrowths are impossible. But the student of astronomy must expect his chief profit to be intellectual, in the widening of the range of thought and conception, in the pleasure attending the discovery of simple law working out the most complicated results, in the delight over the beauty and order revealed by the telescope in systems otherwise invisible, in the recognition of the essential unity of the material universe, and of the kinship between his own mind and the infinite Reason that formed all things and is immanent in them....

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* An Address by Prof. Russell Tracy Crawford, Berkeley Astronomical Department, California, U.S.A., published in 'Publications of the Astronomical Society of the Pacific,' No. 115.

At the same time it should be said at once that, even from the lowest point of view, astronomy is far from a useless science. The art of navigation depends for its very possibility upon astronomical prediction. Take away from mankind their almanacs, sextants, and chronometers, and commerce by sea would practically stop. The science also has important applications in the survey of extended regions of the country, and the establishment of boundaries, to say nothing of the accurate determination of time and the arrangement of the calendar.

It is the intention here to go further than this, and to speak not only of the present, as Prof. Young does, but to go back to the beginning of things and to show the principal accounts in "Our Debt to Astronomy."

Let it be known at the outset that for many centuries, from the beginning of terrestrial affairs, the history of the world is practically the history of astronomy. For many centuries, we cannot say just how many, astronomy was the one and only agent to quicken the thoughts of men and to lead them to a comparatively high state of intellectual development. Place ourselves, if we can, in imagination back into the so-called prehistoric times, and it will at once be evident that the most striking phenomenon of nature—viz., the rising of the Sun, Moon, and stars in the east, their daily journeys across the sky, and their setting in the west—would be the first thing to draw thought and set active minds to speculate. Once started, the keen mind would soon find other phenomena of astronomy to work upon, and in this way was built up that foundation for the superstructure of culture and knowledge which we find in the possession of earliest historical man—our first debt to astronomy.

We find that, in the beginning of what we may call authentic history, astronomy was purely a practical science. Theoretical astronomy there was, wild as it may have been, but it was the practical side of astronomy that was most developed and cultivated. It was very necessary in those times, when agriculture was the main industry, to know when to sow the crops, and when to reap them,—in other words, a knowledge of the seasons was essential to existence itself. And how, indeed, was a knowledge of the seasons to be had, not only then but now, except by a careful study of the wanderings of the Sun from its southernmost position to its most northern station, and back again, during the course of the interval of time that we call a year? Let us consider, in particular, the case of the Egyptians. Egyptian civilization would have been impossible but for the Nile floods, which appear with great regularity. The rising waters reach the region of Heliopolis and Memphis almost exactly upon the day that the Sun is at the summer solstice. It is very evident, then, that the Egyptians before us were under obligations to the astronomers of their age to inform them of the time when the Sun would be at the summer solstice. It is no wonder, then, that the Egyptians commissioned their astronomer-priests to observe carefully for the helical rising of *Sothis or,* as we now call it, Sirius, the dog-star, to give them the necessary datum upon which their year's material prosperity depended. As every advance in early knowledge and civilization was an essential stepping-stone to our present acropolis of culture, we of to-day owe this a second debt to astronomy.

It must ever be borne in mind that our present-day knowledge has not been attained by the spontaneous outburst of genius in a single generation, but has been built up slowly, step by step, from earliest times, each single advance being a necessary precursor of later development. Hipparchus must precede Ptolemy, and Ptolemy must precede Copernicus. Copernicus made the way easier for Kepler, and Kepler in turn added the stone upon which Newton built, and so on. To every contributor of a stone in our Temple of Knowledge, whether it be in foundation or superstructure, we of to-day owe a debt.

Incidentally, we have a decided personal interest in the theoretical astronomy of the Babylonians, who gave us the week of seven days, with the names taken from the Sun, Moon, and the then-known five planets, Mercury, Venus, Mars, Jupiter, and Saturn. Had Uranus and Neptune been known to them, our week would have been nine days long instead of seven, with one day in nine for rest.

To astronomers and their science we owe the evolution of the ideas and conceptions of the size and shape of our globe. The earliest peoples believed the evidence of the senses, and thought that they lived upon a large flat surface. More advanced ideas were that the Earth was a truncated cone, and then a cylinder, and finally, even so early as the time of Pythagoras, it was shown to be a sphere. As to its size no definite conception was had until comparatively recent times. The determination of its dimensions to-day would be impossible but for the astronomer.

Astronomy has not always advanced, but has had its set-backs, from which, however, it has nobly recovered. This brings me to speak of the retarding influence of Aristotle and Ptolemy upon the world's knowledge and culture; its overthrow brought about by astronomers, and the wonderful results therefrom—one of the greatest of our debts to astronomy.

In the third century B.C. Aristarchus of Samos and others of less note had asserted that the Earth was in motion—a very advanced idea for the time; but they could not prove it. Then came a backward step, taken by Ptolemy, who apparently proved that the Earth was not in motion, but was immovable at the centre of the universe. This idea held sway for fourteen centuries, until the time of Copernicus. Its overthrow came at about the time of the downfall of the influence of Aristotle. These two accomplishments were the most vital forces of the Revival of Learning. It has well been said that Aristotle retarded the progress of the world's knowledge and culture more than any other one man. His dictum was the undisputed law in the realms of knowledge for *seventeen centuries*. If anyone had dared to present an idea contrary to Aristotle the sneering question was ever ready, "Do you think you know more than Aristotle?" and the investigator was completely squelched. This absolute servility to the dogma laid down by the great philosopher furnishes one of the most remarkable pictures in history. But sooner or later this had to end, and the end came with Galileo, the astronomer. I may recount here the beginning of this end. Aristotle had said that a large heavy body would fall from a given height in less time than a small light one. For seventeen centuries it had never occurred to anyone to investigate experimentally the truth of this assertion, or else no one had dared to try it. Galileo, the astronomer, however, was not content to take the word of anybody, even Aristotle, upon a matter which could be proved or disproved so easily as this one. Dramatic indeed must have been the scene when the great astronomer mounted to the top of the leaning tower of Pisa and let fall simultaneously a large heavy body and a small light one, and great indeed must have been the astonishment of the populace gathered to witness the experiment when they beheld the two bodies falling side by side and reaching the Earth at practically the same time, demonstrating, as Kipling puts it, that "heart-breaking power, the perversity of inanimate things." At that instant fell also the influence of Aristotle in the scientific world.

Copernicus had just previously shown that the Earth is in motion about the Sun, and when Galileo clinched this idea, so to speak, by turning his telescope upon Jupiter and there beholding the four moons revolving about the central mass, a miniature world, the revolution was complete. It is difficult for us to imagine what a convulsion must have taken place in the minds of men at that time. The Church had taught that the Earth was stationary, and even to think anything else was the greatest heresy, as it was contrary to Holy Writ. In view of this, is it any wonder that we had the Dark Ages? Because of the unswerving adhesion to dogma in those days it is, again, no wonder that the great astronomer, Galileo, was brought before the Inquisition, tried and convicted of heresy, furnishing one of the most dramatic, almost tragic, incidents in the history of science. But the ideas once started by Copernicus and Galileo soon began to spread and found firm supporters, and the world once more started on the way of progress. We cannot begin to appreciate the effects of such a revolution in the world's thought. At first sight one would say, "What a loss of importance and dignity!" We are no longer the great IT,—the centre of the universe about which all else must turn, and to which all else must be subservient. But, on second thought, we would say, and do now say, "What a gain in beauty and grandeur!" We now find ourselves whirling about the great Sun in the infinite sea of ether, gazing into its immeasurable expanse. Can anyone estimate the amount of dollars and cents in this debt we owe to astronomy? No; it is above dollars and cents.

Following quickly upon the steps of these two great astronomers we have Kepler, who gave us his laws of planetary motion, by means of one of which the scientific mind was freed from the so-called perfect path, the circle, and was introduced to the use of the general conic. His work, in turn, formed the stone upon which Sir Isaac Newton built so well. It would be a long story, indeed, to recount the various debts we owe to this man, our greatest astronomer. For the more practical-minded, however, I will call attention merely to one of his accomplishments. In evolving his universal law of gravitation, and in furtherance of his astronomical investigations, it became necessary for him to invent a new branch of mathematics—namely, Calculus. Is there anyone so daring as to estimate the commercial value of calculus? The idea is staggering. It is beyond human power to make the computation. Without this powerful implement, due to astronomy, what could be done in applied mechanics? Where would our sky-scrapers be? How could we build our battleships? Where would our electric cars be? How could our enormous bridges be built?—and so on, almost *ad infinitum.*

Less than a month ago, while coming in from Oakland on the electric car, a friend asked me the old question, in rather inelegant English, "What is astronomy good for?" Among other things I said, "I suppose you see no connection between astronomy and this electric car?" The answer was a ready "No!" The connecting-link is this same calculus.

We find, not only in the applied sciences, but also in many of the natural sciences, higher mathematics to be an absolute necessity. And to what are the higher branches of mathematics due? The answer is, "Principally to astronomy." I wish to quote here from Dr. William F. White's article on "The Nature of Mathematical Reasoning," in No. 609 of 'The Open Court.' He says, "Behind the artisan is a chemist, behind the chemist a physicist, behind the physicist a mathematician." There he stops, but I now add, behind the mathematician the astronomer.

Another great debt that we owe to astronomy is one on behalf of our peace of mind. From the beginning up to the present time there have been, and now are, many people filled with superstition concerning the heavenly bodies. Thanks to astronomy, these are becoming fewer and fewer. In ancient times whole nations were thrown into a panic by an eclipse of the Sun; armies on the eve of battle were commanded to halt, with disastrous results, to await the passing of the ill-omen of an eclipse of the Moon. Comets were supposed to be within our own atmosphere, and to bring pestilence and disease. These phenomena have been shown by astronomers to be ordinary affairs in the celestial mechanism, having no bearing upon our regular existence.

Unusual conjunctions of several planets are even now by some thought to have an important bearing upon the affairs of life. Many crafty people to-day make a livelihood by imposing upon the more superstitious and easily-fooled people by casting horoscopes. One of our daily papers thinks so little of the value of its space as to give up a quarter of a column in every issue to the publication of Cozette's horoscope for the next day.

Within the last few weeks the papers announced that a comet had been discovered by an astronomer on Mt. Ætna, which was coming directly toward us and would soon annihilate the Earth. Immediately after the appearance of this wonderful announcement this observatory had many letters of inquiry concerning it. Some people evidently believed in it, and wanted to know the exact date when the direful catastrophe would occur. Probably there will always be some who will be thus taken in, but, thanks to the teachings of astronomy, we live in practically perfect peace of mind regarding these things. Nations are no longer convulsed by an unexpected eclipse, and astronomers royal are not decapitated for failure to predict them.

A study of the simple elements of astronomy would be very beneficial to artists and writers. They often make serious mistakes through mere ignorance. It is not very uncommon to see an otherwise beautiful painting spoiled completely by a delicate crescent Moon with its horns pointing towards the Sun. A note in No. 383 of the *Observatory* calls attention to the fact that in Chapter I. of 'Jane Eyre,' by Charlotte Bronté, occurs this expression: "(The Moon's) newly risen crescent, attesting the hour of eventide," a statement which makes the author appear ridiculous. Many instances of this kind could be cited.

Finally we come back to Prof. Young's statement, to consider the present-day, all-absorbing, dollars-and-cents value of astronomy. You have all probably travelled at night, resting more or less comfortably in a berth of a Pullman attached to an express train, which in its course thunders by a freight train on a siding. You give but little, if any, thought to it, having confidence in the men running the train. These men in turn have confidence in their running because they are running "on time." Think what it means to the railroads of the country, and to the public, to have the trains running "on time." To be able to handle the enormous traffic of to-day correct time is an absolute necessity. Where do the railroads get their correct time? Another account in our debt to astronomy.

Again, when it became necessary to run the boundary-line between Canada and the United States, astronomers had to be employed. Our maps cannot be made without a knowledge of practical astronomy. Extended surveys also require a knowledge of practical astronomy.

I will call attention to one more, perhaps the greatest, commercial debt we owe to astronomy: that is, the service it renders to navigation. Were it not for the data furnished by astronomers, commerce by sea would practically stop. The sailing-master on the high seas could not determine his position, nor in what direction to head his ship in order to reach a desired harbour. Think what this means in dollars and cents, and estimate it if you can. For this one service alone the science of astronomy is worth more in dollars and cents to the world in one week than has been expended upon it since the beginning of civilization. Do you think that Great Britain, for instance, would take in exchange an amount equal to its national debt for what astronomy gives her? I answer for you, most emphatically, "No."

Even these commercial values sink into insignificance when we consider again, in general, what astronomy has done for us in giving us the laws upon which mechanics is based; in pushing mathematics to its present state of development, the effect of which ramifies rapidly; in being one of the most powerful factors in bringing us to a better understanding of nature; and in broadening our views and taking us out of the narrow confines of a little plain, immediately surrounding the Ægean Sea, and launching us into the infinite realms of space.

In conclusion, I wish to answer one more question—namely, "While some parts of astronomy may be of value, why exert so much thought and energy upon other parts which are purely abstract and theoretical?" The answer to this is that we never can tell to what such abstract speculations may lead, and that, in the end, they may give us something exceedingly valuable, either in the commercial world or in the realms of culture and knowledge. A comparison with prospecting may make it clearer. The miner digs day after day with apparently no return. He may continue to do so with no result, no matter how long he labours; but, on the other hand, at any moment he may unearth a rich vein which will more than repay him for his efforts. So it was with Kepler, when he dug through Tycho's observations day after day with no result, until finally, after much labour and the use of an enormous store of patience, he struck the vein of planetary motion, and his wonderful laws were brought to light. It was Michael Faraday who said so well, "There is nothing so prolific in utilities as abstractions."

And now, in closing, I desire to express the hope that you who have heard what I have had to say, and those who may hereafter read what I have said, will ever feel it unnecessary to ask in inelegant English, "What is astronomy good for?"

Berkeley Astronomical Department,
1907, July 13.

The August Shooting-Stars.

THE atmosphere was pretty clear at the time of the Perseid return this year, and the sky being free from moonlight, successful observations were made at many stations. At Bristol, Mr. Denning watched the sky on August 10, 11, and 12, and found that the highest horary numbers of meteors in the mornings were 45, 36, and 50 respectively. The proportion of Perseids was on the 10th and 11th two-thirds, and on the 12th three-quarters of the total number counted. Generally, the character of the shower was considered to be only of moderate intensity, while the apparent brightness of the individual meteors fell rather below that usually observed. There were some fine objects seen, it is true, notably on August 11 at $13^h\ 17^m$ (observed also at Greenwich), on August 12 at $11^h\ 12^m$, and on August 13 at $14^h\ 10^m$. These were bright flashing Perseids. There were also some large meteors recorded belonging to the sporadic class, or rather to the various feeble showers which are displayed simultaneously with the Perseids. These were directed from Pegasus, Cygnus, Cepheus, Aquila, and other constellations.

Miss Irene Warner, observing at Horfield Common, 2½ miles north of Bristol, obtained observations on August 11 and 12 as under:—

On August 11, watching during 2 hours of the period from $9^h\ 15^m$ to $11^h\ 40^m$, 56 meteors were counted, of which the flights of 9 only were inconsistent with the Perseid radiant.

On August 12, watching during 2½ hours between $9^h\ 0^m$ and $12^h\ 20^m$, 49 meteors were observed, of which 6 were non-Perseids.

The finest meteor of all was seen on August 12 at $11^h\ 12^m$. It was much brighter than Venus, and slowly traversed a short path 2 or 3 degrees east of, and parallel with, the stars α, γ, and δ Persei, where it left a brilliant streak for some seconds.

A large meteor of the erratic class was noted on Aug. 12 $10^h\ 20^m$, shooting from δ to α Cygni and leaving a thick yellow train between these stars.

The very fine object seen on August 12 at $11^h\ 12^m$ at Horfield Common was also seen by Mr. J. P. Kenyon at Davenport, near Stockport. He estimated its brightness, at the moment it burst out near the end of its course, as equal to the Full Moon. A comparison of the observation shows that the radiant was at about $41^\circ\ +57^\circ$ and the height of the object 77 to 44 miles from over Donington, in Lincoln, to Market Harborough, in Leicestershire.

This meteor was also seen by Mr. H. Corder at Bridgwater, who recorded the path from $58^\circ\ +51^\circ$ to $62^\circ\ +47^\circ$.

Mr. Denning observed 5 meteors, including one stationary, during a watch of 40 minutes on August 15 from a radiant at $288^\circ\ +61^\circ$. This represented an active return of a shower seen by the same observer on August 21 to 25, 1879, which gave 56 meteors. Particulars of the observation appeared in the

Observatory for 1879 Oct. vol. iii. pp. 172 and 173, and *Monthly Notices*, vol. xl. pp. 127 and 128.

Mr. J. McHarg, observing at Lisburn on Aug. 12 $9^h\ 15^m$ to $10^h\ 45^m$, counted 60 meteors. Four or five small ones from a radiant near zenith in Draco; nearly all the others were Perseids and one-third of them were brilliant with red trains.

Mr. J. P. Kenyon, of Davenport, near Stockport, watched on Aug. 10, 11, 12, and 13. On Aug. 10, about 10^h, horary rate of Perseids only 5·5, though the rate for all meteors was 30·5. A rate of 5·5 Perseids was obtained again later in same evening. August 11, about 10^h, rate for all meteors 47·5, Perseids 31·5. August 12, about 11^h, rate for all meteors 87, Perseids 49. August 13, 11^h to 14^h, rate for all meteors 42, Perseids 16·5.

Mr. Kenyon saw 2 Fireball Perseids = ½ ☽ and Full Moon thus:—

July 30 $11^b\ 57^m$	12½°	+53°	to	9°	+51½°
Aug. 12 $11^h\ 11^m$......	348¼	+ 5	to	343½	− 3½

The Alleged Summer of 1907.

Summer, according to the ordinary almanac, includes only the months of June, July, and August, and it does occasionally happen that the actual meteorological summer agrees absolutely with the almanac—for instance, in 1877 and 1887, when the thermometer at Greenwich only once reached 70° outside the three summer months, although that limit was exceeded 71 and 78 times respectively within those months, both summers being warmer than the average, especially the Jubilee summer.

On the other hand, the same limit of 70° has been reached as early as March 24 (in 1871) and as late as October 15 (in 1861 and 1862), while the period in any year from the earliest to the latest of such readings has varied from 93 days in 1887 to 191 in 1848.

But to come to the present year. It seemed probable until the last week in September that this year would join the select few in which the thermometer reached 80° in May only. This happened in 1841 (May 27 and 28) and in 1862 (May 6), while in 1860 the highest temperature, though less than 80°, was also reached in May (76°·5 on May 23). The reading of 81°·5 on May 12 stood for nearly four and a half months as the highest for this year, though September had made some amends for the deficiency of the calendar summer.

Premising that the Greenwich records date back to 1841 inclusive, the mean temperature of the "summer" of 1907 holds a very low place, though redeemed from the lowest by an improvement in August. The actual figures for the three months were 56°·5, 58°·6, and 60°·5 respectively, giving a mean for the "summer" of 58°·5, practically equal to (though to the next place of decimals warmer than) 1879 and 1888, and only exceeded in coldness by 1841 (58°·0) and 1860 (57°·4), these being the only years in which the summer temperature was as much as 3° below the average (65 years). There have been six colder Junes, including one so recent as 1903, and four colder Julys, 1841, 1860, 1879, and 1888, the same years in which the summer temperature was lowest. *Perhaps* there will be a cold July and a cold summer in 1926. If so, the short interval, 9 years instead of 19, between 1879 and 1888, will have to be explained away as an "inversion," but we will leave this difficulty to others.

Three of the four previous coldest summers, and especially 1860, were followed by cold Septembers, the exception (1841) having a September just above the average, a phenomenon (likely to be) repeated this year, and in 1841 the mean temperature of September was the same as that of the summer, in which respect also 1907 will probably be found to resemble 1841. Septembers warmer than the preceding summer are not unknown. The well-remembered September of 1895 was only 0°·1 cooler than the summer; in 1843 and 1890 September was 0°·4 warmer than the summer, in 1898 0°·5 warmer, and in 1865 1°·6 warmer, in spite of the fact that the summer of 1865 was nearly a degree warmer than the average. 1865 was a very exceptional year, the number of days on which the thermometer reached 70°, 132, being far in advance of any other year, the celebrated hot summer of 1868 only providing 108. In 1865, moreover, but for 0·16 inch on September 21, there was no rain from August 29 to October 7 inclusive, making September 1907 seem damp in comparison. A September drought occurs on an average once in about four years, but the occasions are not well distributed. The following are the dates since 1841 inclusive when at least 14 consecutive days, the period being comprised wholly or partially in September, were without "rain" at Greenwich: 1850, 1851, 1854, 1855, 1865, 1868, 1869, 1870, 1873, 1886, 1889, 1890, 1900, 1902, 1907. In 1869 most of the period was in August, but in most of the other years there was an actual "drought" in September itself, though occasionally lasting over one end of the month. In 1865, as noted above, there were really two "droughts" with a break of a single day.

So recently as 1898 September provided less rainfall than did September this year before the drought began, and in 1898 most of the rain that month fell on the 29th and 30th. 1895 September would have been drier still but for a heavy rainfall of 0·8 inch on the 7th.

The summer of 1907 was not "wet." June, thanks to an exceptionally heavy fall on June 29, exactly a year after a still heavier fall last year, provided an excess above the average, but August, and especially July, were deficient, the total deficit on the three months being an inch and a quarter. The drought of *September* 1907 lasted nineteen days, no rain falling between the

6th and the 26th, so that the light rain early on the latter morning prevented the setting up of another "record," the nineteen days having been exceeded at this time of year in 1865, 1868, and 1870, and equalled in 1851, 1855, 1869, and 1890, the actual largest number of days of drought being for this season 24 in 1870, while of the more common spring droughts the longest was of 30 days in 1893, though 1852 holds the unique record of two droughts of 28 days each, with only two wet days between.

During the first 20 years of Greenwich registers, the highest temperature of the year occurred only once later than August 2, the actual latest being August 10, in 1842. The next 30 years provided several later instances, the latest being August 21, in 1883. Since the completion of the 50 years, however, we have had the maximum on August 31 last year, on September 8 in 1898, on September 24 in 1895, and now this year on September 25, thus establishing another curious record, which, however, does not appear to have been confirmed at Westminster. The shade-temperature at Greenwich on September 25, the last day of the "drought," reached 82°·7, a figure only exceeded so late in the year during the phenomenal heat-wave of 1895, when the reading exceeded 80° on each of six consecutive days—September 23 to 28.

If an explanation is sought for the apparent shifting of what might be called the "heat-apex" for the year, it will be found necessary to collate records from places whose surroundings have not been so steadily modified for many years as have those of Greenwich Park by the expansion of London. W. W. B.

The Sun-spots of 1907 *June.*

The sun-spot history of the month of June 1907 is soon told. It is practically the history of a single group, No. 6205 in the Greenwich series. This was one of the finest groups seen during the present maximum; the other spot-groups of the month were few in number and unimportant in character and size.

Rotation 719 began June $24^{d}\cdot634$.

Notes on the Principal Spot-Groups.

Group 6205.—A straight and almost continuous stream, consisting chiefly of three very large spots, nearly equal in size and close to each other, beside a multitude of small attendants. The leader was, as usual, the best-defined member of the stream and was nearly circular; the rear spot was intersected by a great number of bright bridges. Area of the group on May 20 about 2000.

Spot-Groups in 1907 June.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1907.	1907.	days.	d	°	°	
6198	June 1	June 4	4	June 7·7	224	+15	Small spot.
9	2	9	6	4·8	263	−11	Faint spot. Intermittent.
6200	5	7	3	5·4	255	−11	Two small spots.
1	8	17	10	15·0	127	−19	Regular spot.
2	10	21	12	16·1	113	+12	Irregular stream.
3	13	17	5	14·8	130	+ 7	Two small spots.
4	13	19	7	15·9	116	−21	Small cluster.
5	13	25	13	19·6	66	−15	Very fine stream.
6	17	17	1	15·2	124	−19	Small spot.
7	24	24	1	20·9	49	− 5	Very small cluster.
8	25	27	3	21·8	37	− 8	Very small spot.
9	25	July 1	7	July 1·3	272	+ 4	Small unstable stream.

E. Walter Maunder.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Transits of Mercury.

Gentlemen,—

Now that a transit of Mercury is again approaching, a few words may be interesting on early observations of that phenomenon. Some medieval writers fancied that the planet was seen to pass over the Sun in the year 807 (during the reign of Charlemagne), but the observation in question was doubtless of a large spot which was visible on the Sun's disc during eight days, the writers being ignorant (not to mention other considerations) that the planet's transit could not last anything like that time.

The first person who really witnessed a transit of Mercury was Gassendi, who observed one at his native town, Digne in Provence (now included in the department of Basses Alpes), on the 7th of November (N.S.), 1631, shortly after his return from Holland.

The next person to witness a transit of Mercury was our countryman Jeremiah Shakerley, who observed that of the 3rd November, 1651, at Surat in India, whither he had gone because it would not be visible in England. That of the 3rd of May, 1661, was observed by Hevelius at Danzig and also by several observers in England.

But in several respects that of the 7th of November, 1677, was of especial interest. Halley observed it at St. Helena on his famous expedition to form a catalogue of stars in the southern hemisphere. The accuracy with which, it seemed to him, such observations could be made led him to suggest that those of Venus, when in a similar position, would afford a peculiarly advantageous means of determining the solar parallax. This was accordingly acted upon by his successors, the first occasion taking place in 1761, when another future Astronomer Royal was one of the observers. Admiral Smyth points out, in the 'Speculum Hartwellianum' (p. 43), that the expense of this was provided, not by George III., but by George II., as the arrangements were made before the death of the latter, and the King's warrant is, in fact, dated the 21st of July, 1760. He also rather startles us by calling attention to the amount of liquor which forms an item in Maskelyne's bill at St. Helena and during the voyages to and from that island; it certainly proves that he was, as the Admiral remarks *, not quite what is now called a teetotaller, and indeed almost reminds us of Prince Henry's remark about the sum charged for sack in Falstaff's tavern-bill.

But the point about transits of Mercury to which I desire to draw special attention is their periodicity. Between the transits observed by Gassendi and by Halley intervened a period of forty-six years. And at that interval after a transit there must almost always be another at the same node, because 46 revolutions of the Earth are completed in almost exactly the same period as 191 revolutions of Mercury, the former comprising 16,801 and the latter 16,802 days. We have all often heard of the great inequality of Jupiter and Saturn and the long inequality of Venus and the Earth; but I do not remember having seen any allusion to this near commensurability between the orbital motions of the Earth and Mercury.

The transit forty-six years ago, on the 11th of November, 1861, was observed at several places in the north of England, as well as on the Continent; but the weather appears to have been cloudy at Greenwich. The ingress of that which followed it, on the morning of the 5th of November, 1868, took place in England before daylight; but observations of the egress were made at the Royal Observatory by several persons, including the undersigned, and will be found in the 'Monthly Notices,' vol. xxix. p. 11. Both ingress and egress were observed in Australia (p. 89). Forty-six years after that another will occur, in November 1914, which will be the next after the one we are now looking forward to, on the forthcoming 14th of November, the whole of which will be visible in this country if the sky be clear.

Yours faithfully,

Blackheath, 1907, Sept 5. W. T. LYNN.

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* Prof. Newcomb repeats this remark in his 'Reminiscences of an Astronomer,' having met with a copy of the 'Speculum,' now somewhat rare.

The Loss of Energy by the Sun.

GENTLEMEN,—

It would indeed be interesting if the riddle of the Sun's radiation could be evaded in the way suggested by Mr. Monck in the September number of the *Observatory*. Reasoning from our accepted views of physical laws, and the infinity of space, the energy radiated from a hot body like the Sun must proceed through the ether in straight lines for ever, unless it encounters some material obstacle. Such being the case, we must suppose that much the greater part of the energy radiated by the Sun during all the ages of its existence is still pursuing its way at the rate of 300,000 kilometres per second in the infinite void. The question whether the difficulty we meet with in accepting this view could be avoided in the way suggested by Mr. Monck has frequently occurred to me. But I have never been able to make any such hypothesis consistent with observed facts. The law, if such it be, ought to apply to terrestrial as well as celestial objects. But we know, from the changes of atmospheric temperature, that our Earth during a clear night loses heat by radiation into space without there being any object nearer than the fixed stars to intercept it. Moreover, if the Sun is to radiate heat only towards the Earth, or, conversely, if the Earth is to furnish by its direction and angular magnitude in space the incentive to the radiation, will the latter be governed by the Earth in its actual position at any moment, or in its position 8 minutes later? Neither supposition seems easy to accept. Moreover, the Moon varies its angular velocity around the Sun. How can we suppose that the Sun sends forth its radiation in such a way as to exactly hit the Moon in its varying course?

The same question will apply to the fixed stars radiating energy to the Earth in the form of the light by which we see them. We do this by the aid of light which has been several years in coming. Can we suppose that the light would not have been emitted but for the existence of our Earth? I need not pursue these questions further: if the reader sees any way of answering them without supposing that every incandescent body, whether cosmic or terrestrial, radiates energy in all directions, irrespective of the existence of other bodies, he may solve the riddle.

Washington, SIMON NEWCOMB.
1907, Sept. 14.

Linné.

GENTLEMEN,—

I write in the hope of leading to some elucidation of the mystery of this crater. Is it now a ring? and if so, of what dimensions? And what relation do it and the white spot bear to the hill? Is there more than one hill?

Professor W. H. Pickering has made a very good investigation with reference to this object, as far as it goes, in Harvard Annals, vol. xxxii. p. 199; but does not clear up the above points. It has long been the general, though perhaps not the universal, opinion of astronomers that Linné underwent a decided change in or before the year 1866, ceasing to be a conspicuous crater, W. H. Pickering accepts this as a fact, holding that the crater had been 10 or 11 kilometres in diameter, while it is certain that no such conspicuous crater has been seen since 1866.

But the evidence as to the slight ring is conflicting. Many observers saw an "extremely shallow crater," or the "ghost of a ring," in 1867 and 1868; and Mr. Pickering holds that this is not now visible in a 15-inch refractor, and concludes that there must have been a further change since 1868. Some observers say that at that time it was 8 to 10 kilometres in diameter, or nearly as large as the original crater; which would thus appear to have become filled up nearly or quite to the brim, or its walls may have subsided. Some observers, however, do not give the dimensions of the ring, so that there is no evidence as to whether they all saw the same object; my own opinion is that they probably did, and that it was the same that I have seen with a 4¼-inch refractor many times, commencing with 1867, 3 mo. 12, and continuing up to the present year. W. H. Pickering sees (p. 201) Linné "situated on the N.E. edge of a whitish ring 16 in. in diameter," "visible throughout the lunation." I have never seen this ring, nor am I aware that it has been noticed by anyone else; it is four times the diameter of the other ring. On the other hand, W. R. Birt in 'The Mare Serenitatis,' published in 1869, gives the diameter of a ring as only 2·5 miles from ridge to ridge.

How is it that the ring is invisible with a 15-inch telescope? Is it not a true ring, but only an appearance caused by the low optical power used? Can it be that the shadow of the hill, or the small craterlet which has always since 1866 been visible with large telescopes, can become so diffused with smaller instruments as to appear like a dusky circle on the white spot? I do not see that this can be the case, especially as Knott, who saw the ring in 1867, observed with a 7⅓-inch refractor, and Dr. Huggins saw it with a power of 500.

The ring has never been visible close to the terminator, but, so far as I know, it is still visible under the same circumstances as ever since 1867. With a rising Sun it begins to be visible when the terminator reaches the "strait" between the Apennines and the Caucasus, and ceases to be brighter than its interior when the Sun has about half risen on Autolycus and Aristillus, thus assuming its usual appearance of a white spot. About sunrise and sunset I always see Linné as a hill; but the strange thing is that the hill is always seen on the sunward side of the ring. Are there two hills? Or can the appearance be explained on the supposition that the section of Linné is somewhat as drawn in Birt's 'The Mare Serenitatis,' but perhaps larger, and with such a steep and narrow inner slope that it cannot be detected in a 4¼-inch telescope, and such a gentle outer slope that it only gives the appearance of a hill when the Sun is shining well upon it?

As regards the white spot, W. H. Pickering has brought forward fairly conclusive photographic evidence that it has become less conspicuous since 1868, and this change he thinks, is probably still going on. Yours faithfully,

West Hendon House, Sunderland, T. W. BACKHOUSE.
1907, Sept. 18.

The Priming of the Tides.

GENTLEMEN,—

During a summer holiday at Sheringham, on the Norfolk coast, my attention was drawn to a fact relating to the Tides of that region which seems to have escaped the notice of the residents, and the theoretical possibility of which, although quite obvious and interesting, is not alluded to in any book that I know of.

Put in its briefest form, the fact is as follows:—On the Norfolk coast there are more than twice as many low tides *in the middle of the day* as there are high tides, and the fact can easily be substantiated by counting on the Tide-Table the number of High Waters that occur between, say, 12 and 4 in any fortnight and the number of High Waters that occur between 6 and 10. (The time of Low Water is not given.) Speaking roughly, one finds that in any fortnight there are eight days with Low Water between 12 and 4, and only three days with High Water between those hours. The fact to which I am calling attention is not the irregularity in the recurrence of the time of High Water, but the exceedingly convenient time of day at which the largest number of low tides occur. The advantage to the public was borne in upon one when on any of three inconvenient days one saw all the children driven to take refuge on the bank of flints under the cliff from 11 o'clock onwards, and found the sands were unpleasantly wet to walk on till 4 or 5 o'clock in the afternoon.

The machinery behind it all is, of course, only the well-known priming of the tides at Full and Change, or rather during Spring-tides, which in England occur about 3 days after Full and Change of the Moon. It has nothing to do with the season of the year nor with tidal currents, nor yet with the interference of meeting tides such as one finds in the Solent. For the week-end tripper and the summer visitor it has a very real advantage, and as I heard that a local genius was being employed to write an appreciation of Sheringham for the purposes of advertisement, I made him a present of the discovery. If he only understood his business he ought to make quite as effective a head-line out of "The Priming of the Spring-tide" as out of "Poppyland" and "The Garden of *Sleep.*"

The graph obtained by plotting the length of successive Tidal Days does not seem to be as generally known as it deserves to be. Its broad valleys and sharp peaks show clearly that when Tides prime they do so little and long, but when they lag they lag a lot. They remind one of the Frenchman who said, "Ce que je sais je sais mal, mais ce que j'ignore je l'ignore parfaitement." For quite a week the usual 50-minute interval is about 30 minutes or less, and if this week begins when low water occurs at about 12, low water will not be later than 4 at the end of the week. This priming happens at Spring-tide, and so one only wants to find a locality where the Low Water of Spring-tides occurs at 2 o'clock, and one will then get the full benefit of the priming in the middle of the day, and the sands will be far more accessible than one has a right to expect. Such was my inexperience in these matters that it never occurred to me to determine how near we were to Spring-tides by any other means than looking out the Moon's age. But when I asked a fisherman when next Springs would occur, he replied, "High Water to-day is about 6, so the tides are half-sprung, we reckon to get the highest tide at 8 or 9." He went on to give me his personal opinion that the 9 o'clock tide was the highest, though some held that the sixth tide after Full and Change was "full-sprung." That, he said, would occur at about 8.30, and he considered that the seventh flood was the highest, and that would be a 9.0 o'clock tide. Of course it is easier when one's attention is confined to a single locality to connect Springs and Neaps with the time of day at which High Water occurs.

Thus the Norfolk coast has this double advantage over places like Margate and Littlehampton. It gets the full benefit of a great number of low tides, and those tides are Springs, and therefore the very lowest of all. I can remember how we looked forward as children to the very low tides at Cromer when the rock was exposed which was said to be part of the old church of Shipton. We should have lost all that had the establishment of the region been such that Springs low water occurred at, say, 8 o'clock.

Yours faithfully,

Crowthorne, 1907, Sept. 11. J. A. HARDCASTLE.

Determination of Latitude, &c.

GENTLEMEN,—

In the method of determining latitude, azimuth, and time which Prof. Pearson describes in the current number of your Journal, I understand that the main object is to allow the surveyor to observe on the vertical circles of the brightest stars at a time when very few are visible.

Perhaps, however, this will not preclude him from placing one of the two circles near the meridian, so as to observe a fast star for time and a slow one for azimuth; and if these conditions are secured, it only remains to consider the method in relation to the determination of latitude.

In a note by P. H. C. in the *Observatory* for June 1898 it is pointed out that a transit-instrument's position may be represented by latitude and longitude; and from this point of view there are two distinct conditions for accurate determination of the observer's latitude:—

(1) The latitude of the instrument will best be found if the axis be placed nearly in the meridian of the star observed. This is not quite the same as the rule sometimes assumed that the position to be chosen is the meridian of the observer.

(2) The hour-angle or longitude of the instrument must be accurately measured if the observer's latitude is to be determined.

To illustrate these two points I have recently observed ζ Cephei at its eastern elongation, using a 3-inch transit-instrument with five threads, and observing four transits over each by Struve's method.

The result showed that condition (1) was complied with, and it seemed as if the observations obtained from any one thread alone would have sufficed to determine the latitude of the instrument within a tenth of a second of arc.

But condition (2) was not complied with. It appeared that an error of one-third of a second of time in the hour-angle would produce an error of two seconds of arc in the latitude of the place. In order to comply with condition (1) I had set the instrument so that in the south part of the field the star's horizontal motion should be reduced to zero; and it would be necessary to choose an azimuth farther north if the star's velocity was to be high enough for a good determination of the hour-angle.

ζ Cephei being six degrees from my zenith at culmination is too far north to secure the observance of the conditions (1) and (2). But the effect of the error in longitude diminishes rapidly as we approach the zenith. There are five Nautical Almanac stars which pass within three degrees to the north of the zenith, and it seems likely that the observation of any one of them would satisfy conditions (1) and (2) better than any observations made in the southern sky. The two methods would be alike in avoiding the necessity of applying a correction for refraction.

Uplands, Cobham, Surrey,
1907, Sept. 26.

Yours faithfully,
WALTER HEATH.

Comet Daniel (1907 *d*).

GENTLEMEN,—

I observed this Comet on Aug. 4 and on a number of subsequent dates, but was unable to examine it in the telescope. On Aug. 4 I estimated nucleus equal to 2½ mag. and the tail, spreading out to west, could be faintly traced over 3°.

On later mornings, during the third week in the month, and before moonlight interfered, the tail had visibly increased to a length of about 5° and was probably longer than that, but the extreme faintness of the outlying portion made it difficult to assign definite limits. The nucleus exhibited little, if any, increase of brightness after the first week in August, but the conditions were more favourable for observation at that period than on subsequent dates.

It has been stated in various publications that this comet is the brightest that has been presented to us since 1882, but it is certain that several comets which appeared during the last 25 years were more conspicuous and showed a greater extension of tail.

Yours faithfully,

Bishopston, Bristol,
1907, Sept. 10.

W. F. Denning.

OBSERVATORIES.

National Observatory, Paris.—The lamented death of M. Bossert, chief of the computing staff, at Paris, necessarily caused some changes of position in the staff of the Observatory. M. Renan has been promoted to the rank of "astronome titulaire," and M. Gonnessiat, formerly of the Lyons Observatory, who was a member of the expedition charged with measuring a degree of the meridian in Peru, has joined the National Observatory as "astronome adjoint" of the first class, and will be attached to the department of the meridian circle "du Jardin," which is now evidently considered one of the most important instruments of the establishment. It has been announced in former reports that this instrument has undergone several modifications and improvements lately, one being that it has had new circles attached, and the division-errors of these are to be determined very elaborately by the method devised by M. Loewy, and published by him in the *Comptes Rendus.* A self-registering micrometer for recording transits has also been installed on this instrument, and also a micrometer for registering declinations, both made by the firm of Gautier. The transit-micrometer is of the form in which the screw moving the transit-wire is turned by an electric motor, the speed of the wire being regulated by a differential movement under the control of the observer. It is said that the experimental observations show no difference of personal equation between two observers who have used the new apparatus.

Photometry is a new branch of research that has been taken up at Paris. This work is in the charge of M. Nordmann, and has for its end the determination of the relative absorption by the atmosphere of light of different wave-lengths. The absorption of homogeneous star-light has been studied previously by others, but this is believed to be the first attempt to do this for different parts of stellar spectra, although a similar research has been made in the case of the Sun. M. Nordmann has already got indications that the red and orange part of the spectrum of stars is much more absorbed than the blue and violet.

Reference is made in the Report to the publication of Astrographic Circular No. 12, which completes the share of Paris in the Eros campaign, and to the publication of the Astrographic Chart plates, in the course of which work a small planet and some interesting variable stars have been found.

The Grand Instrument méridien and the Gambey instruments have been used to determine the coordinates of the reference-stars of the Astrographic Catalogue, 5000 observations having been made with the former and nearly 4000 with the latter. The Great Equatorial of the West Tower has been used under M. Bigourdan's superintendence for making observations for the determination of the aberration-constant by M. Loewy's method, and the other equatorials have been used for miscellaneous purposes.

PUBLICATIONS.

'MICROSCOPY: THE CONSTRUCTION, THEORY, AND USE OF THE MICROSCOPE'*.—Strangely uneven, this book is notwithstanding, in our humble opinion, one of the best in the English language, and one that is certain, therefore, to take a high place in the literature of the microscope. Obviously the work of a thoroughly practical man, well in the van of the army of workers now devoted to the subject with which it is concerned, it at the same time deals with the more difficult theoretical problems in an illuminating and suggestive manner. The author has wisely, we think, broken with the time-honoured custom of attempting to deal in the same book, not only with the microscope, but with its "revelations" also. When the science of microscopy was young, and the total amount of knowledge concerning it small, there may have been no objection to this course, but the time has certainly arrived when it is no longer desirable, even if possible. To adopt a somewhat colloquial but very expressive classification, devotees of the microscope can be sharply divided into two groups —brass-and-glass men and bug-and-slug men. To the former the microscope is chiefly of interest as an optical instrument pure and simple; to the latter it may be nothing more than a necessary and ill-understood tool for the performance of biological or other work. Such, however, is the complexity of the instrument and the difficulty of its manipulation, that it may safely be said that its use in an intelligent and efficient manner is practically impossible without a sound, even if elementary, knowledge of the principles involved in its construction and use. The most ardent biologist or botanist without this knowledge would probably find it impossible to get more, if as much, from an apochromatic objective costing £15 as the worker better informed as to these optical principles would obtain from an ordinary achromatic objective costing 15*s*. A book therefore such as we are now dealing with appeals to both these classes of workers.

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* By Dr. Edmund J. Spitta, President of the Quekett Microscopical Club, &c. Published by John Murray: London, 1907. Price 12*s*. 6*d*. nett.

The introductory chapter of Dr. Spitta's book we must at once say is the worst: it disfigures the book. Its scope and method of treatment are both wrong. Commencing with a definition of simple and compound microscopes which leaves much to be desired, it then proceeds to deal with the optical centre of a lens; but we submit that to show the position of this centre in the various forms of lenses in half a dozen figures, as is done, without at the same time explaining the properties of this point and its relations with the more important cardinal points of Gauss, is to carefully load and aim a gun and then forget to pull the trigger! Worse, however, follows. Fig. 16 shows a very thick lens bringing parallel rays to a point; the distance between this point and the optical centre is carefully indicated and labelled "true focus"! Again, as examples of the optical terminology employed, we have a focus (a point) and the principal focal length, indifferently referred to as "focus of the lens," "actual focus," "equivalent focus," "true focus," "solar focus," and "principal focus," and such loose expressions as "this is easily understood as the natural results of the working of the conjugates." The second and third chapters deal mainly with the mechanical parts of modern microscopes, a feature being the number of figures showing various modern fine adjustments, whilst in chapter four we have the various aberrations of the objective and the principles involved in their correction dealt with in a very clear and instructive manner. We would make one suggestion here for the benefit of future editions—a few aberration curves might be given with advantage.

As regards the relationship existing between the numerical aperture of an objective and its resolving power as dependent upon diffraction, which is dealt with fully and satisfactorily, we have always thought that it might be presented to the student with advantage from the historical point of view. Fraunhofer in making his celebrated experiments on diffraction, at the beginning of last century, investigated with great care and skill the diffraction phenomena produced when parallel light falls normally upon a grating. He discovered and enunciated the law which determines the angular separation of the diffracted beams, and showed that when the distance between adjacent spaces of the grating is equal to, or less than, a wave-length of the light employed for illuminating purposes, then no diffraction can take place. From this he drew the remarkable conclusion that so long as the inequalities of a surface of glass were of the order of a wave-length or less that surface would both transmit and reflect light perfectly without any dispersion due to diffraction, and, to use his own words, "it would therefore be impossible by any means to render these inequalities visible." In a footnote he adds, "from this we may conclude what it is possible to see through microscopes. A microscope object, for instance, the diameter of which is equal to *w* [the wave-length of the light employed] and which consists of two parts, can no longer be recognised as consisting of two parts. This shows us the limits which are set to vision through microscopes." Here, then, as Rayleigh has pointed out, we have a clear anticipation of the diffraction theory of microscope vision. It should be remembered that Fraunhofer was considering normally incident light only; if he had proceeded to consider the effect of obliquity he would doubtless have arrived at the modern half-wave limit to the separating power of a microscope system. Approached from this point of view, there is no mystery about the way in which an immersion system increases the resolving power of the microscope. *It does so primarily by lessening the wave-length of the light falling upon the immersed object,* and this, as Dr. Spitta says, results in the "bunching" together of the diffracted beams. Comparing a dry with an oil-immersion objective taking up in air and oil respectively the maximum cone of 180°, it follows that, using the same light for both objectives, the resolving power of the immersion objective must exceed that of the dry one in the ratio of $n:1$ (n being the refractive index), since it is in this ratio that the oil reduces the wave-length of the light employed. The limit thus set to the resolving power of a microscope by the refractive index of the front lens and of the immersion medium shows, it may be remarked in passing, that could an optician avail himself of the most refracting transparent medium known—the diamond—the present visual limit could be nearly doubled. This limit, it is interesting to know, has for photographic purposes been practically reached by the monochromatic ultra-violet microscope of Zeiss, in which light of half the wave-length of that occurring in the most visible part of the spectrum is employed. We notice that Dr. Spitta in considering this shortening of the wave-length which takes place in the immersion medium considers it necessary to enter into an explanation as to why the colour is not changed. This matter presents no difficulty if it be remembered that the sensation of colour depends directly upon the frequency of the ether-waves, and this frequency is of course constant, no matter what media the waves may pass through. There is another point which we think should have been brought out in the discussion of an oil-immersion objective. We are afraid that the student may be left with the impression that the brightness of the image of the source of light seen in a microscope may be more or less indefinitely increased. It would have been well to have made it clear that no matter what optical system is employed, even when the pupil of the observer is filled with light, the image of this source can never appear to be so bright as the source itself seen directly.

The chapter on "Depth of Focus" is not very clear, largely due to the fact apparently that some of the reference-letters in fig. 84 *a appear to* have been misplaced.

The chapter on "Eyepieces" brings out the optical action of the different forms of eyepieces simply and clearly, but it is scarcely legitimate, in order to determine the diameter of the Ramsden circle, to start with the assumption that the compound microscope "produces the same effect as would be shown by an optically perfect lens of the same N.A. and magnification." The demonstration in the footnote leaves nothing to be desired.

In considering the subject of "Optical and Mechanical Tube-Lengths," Dr. Spitta shows up in a strong light the inconsistencies of the various makers. It appears from what he says that whilst there is not much difficulty in adapting eyepieces generally so that their lower focal planes occur at a fixed distance from the upper end of the mechanical tube, it is practically impossible to do the same thing for the upper focal planes of objectives. In these circumstances would it not then be better to clearly mark (1) each objective, not only with its focal length, as is now done, but also with the optical tube-length for which it is corrected, and the position with respect to the shoulder of the mount, of its upper focal plane; and (2) each eyepiece with its equivalent focal length? Were this done, any information which the worker wants respecting magnification could be easily determined with a foot rule.

We are sorry to find that the term "aplanatic cone" has been adopted in connection with substage condensers. The coining and adoption of this term does no credit to English opticians. We already have the word "aplanatic" in use to denote two different things. In its earlier use it meant simply absence of spherical aberration; in the sense introduced by Abbe it is restricted to lens systems satisfying the sine condition. In its latest use, as applied, for instance, to a chromatic Abbe condenser, it is absurd, because, as determined, it depends not only upon the breadth of the source of light, but also upon its depth in the line of sight, as well as upon the thickness of the slip employed. It would have been much better, we suggest, to use some such term as "efficient cone." The explanation of the efficacy of oblique light in resolving fine structure is dealt with in what is to us an original and striking way. Briefly it amounts to showing that when oblique light is employed it enables the objective to get a better grip of the diffracted beams necessary for resolution. It is not clear, however, how it happens that oblique light in *one* azimuth only is more effective than oblique light in *all* azimuths, as must apparently be the case when the whole of the condenser-aperture is in use.

The chapter dealing with the testing of objectives, which extends to some fifty pages, is undoubtedly one of the best and most valuable in the book. It should alone make the book a success. It deals largely with the use of the Abbe test-plate, which the author refers to as a test of "far more searching character" than the old time test-objects. For the author's purpose, the different objectives are classified and dealt with in detail, full instructions being given as to how to test, what to look for, and how to interpret what is seen. Tests with the aid of the more usual test-objects—diatoms, &c.—are dealt with in the same masterly and satisfactory manner. The text of this chapter is illustrated by some 15 or 16 excellent full-page plates of half-tone reproductions of photographs (oh, that they were the photographs themselves!). This chapter might well be published as a separate brochure.

In two of the final chapters, written, as we understand from the preface, by Mr. Conrady, the subject of "Theories of Microscopic Vision" is dealt with. Written by such an admitted master of the subject, they form a very valuable addition to the book, consisting, as they do, of a lucid presentation of the subject so far as it has been developed. We are told that "the real difference between Abbe's theory and all others is that whilst the latter start from the object and *assume* this—consciously or unconsciously—to be self-luminous, the former begins at the source of light." Referring to the recent attempts that have been made in England to show that the resolving power of the microscope is greater than that arrived at by the application of the Abbe theory, it is suggested that so long as their authors do not supply either a satisfactory mathematical proof, or, better still, convincing visual and photographic demonstration of their conclusions, no great importance can be attached to them—a conclusion from which few will dissent. F. J. C.

PERTH, WEST AUSTRALIA.—Since writing the notice published in our July number (p. 289) we have received from Mr. Cooke a catalogue of 420 standard stars, the first astronomical volume issued from the Perth Observatory. In an accompanying letter he explains that he thinks it advisable, in making his catalogue of reference-stars for the astrographic work, not to use the equatorial stars of the *Nautical Almanac* for clock-error, nor to find the nadir-point by reflection from mercury, but to use standard stars in the zone under observation, 32° to 40° S. Dec. Also he proposes to confine his meridian observations to this zone, and to observe and re-observe exactly the same stars every 10 or 12 years, a plan which is already adopted in some degree at several observatories. The last Paris Catalogue, for instance, is the re-observation of the stars of an earlier one, and Greenwich continues year after year to observe the stars of the *N. A.*

ALTITUDE TABLES *.—One of the books with which a navigating officer is well acquainted is known as 'Burdwood's Tables,' which gives the Sun's true bearing or azimuth for every four minutes of the day, the ship being within certain parallels of latitude. It occurred lately to the Rev. Frederick Ball, a Naval Instructor in H.M. Navy, that it would lessen the work of the practical navigator, and increase the probability of accurate results being obtained from his observations, if there were a companion volume which similarly gave altitudes. He consulted the Astronomer Royal, and the result is a book of Altitude Tables for intervals of four minutes of time, the place of observation being latitude between 31° and 60° and the object having declination not greater than 24°. The computations were made unofficially by four members of the established staff at Greenwich under the superintendence of Mr. Crommelin, to whom Mr. Ball offers his grateful thanks for many suggestions and zealous cooperation. The actual formula used was, we believe, one first published in these pages * by Prof. Turner in the year 1894, and which Mr. Crommelin found very suitable. A comparison volume for latitudes from 0° to 30° is in preparation, and will be published by the end of the year.

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* J. D. Potter, 145 Minories, E.C. Price 15s.

NOTES.

Comet Notes.—Accounts continue to come in of the appearance of Comet Daniel in August; *Ast. Nach.* 4200 contains photographic observations of its spectrum by H. Rosenberg, of Göttingen, August 9, 11, and 14. A continuous spectrum extended from wave-length 505 to 370; superposed on this were two broad bands at 473 (the 4th carbon band) and 388 (the 3rd cyanogen band); these bands were some 3′ long; four smaller and fainter bands were noted at 4380 (perhaps the 5th carbon band), 4224 (the 2nd cyanogen band), 4055, 4035.

The same number contains a continuation of Dybeck's ephemeris by Dr. Franz:—

	R.A.			Dec.			R.A.			S. Dec.	
	h	m	s	°	′		h	m	s	°	′
Oct. 3 ..	11	37	44	3	21 N.	Nov. 4	13	12	24	3	55
7 ..	11	52	28	2	13	12	13	29	56	5	11
11 ..	12	6	10	1	9	20	13	45	46	6	14
15 ..	12	18	58	0	10 N.	28	14	0	3	7	8
19 ..	12	30	58	0	46 S.	Dec. 14	14	24	29	8	26
27 ..	12	52	52	2	28 S.	30	14	43	48	9	9

The comet will remain a morning object throughout, its brightness diminishing from 3·0 Oct. 3 to 0·2 Dec. 30.

Extracts from Sydney newspapers have come to hand showing that the comet excited much attention in Australia. It was observed by Mr. Merfield on July 30, when it was of the 4th magnitude, with a tail 2½° in length.' It seems that no orbit or ephemeris of this comet was telegraphed from Kiel to Australia, the result being that much uncertainty prevailed there as to the nature of its orbit, and the question of its identity with that discovered by Prof. Daniel in June. All this uncertainty would have been saved by a few telegrams, and it would certainly seem advisable to send these in similar cases in the future. It may be justifiable to spare the expense of telegrams in cases where a comet is very badly placed for southern observers, but such was not the case here, and the slightly more advantageous position for northern observers might easily be balanced by better weather in the south.

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* Vol. xvii. p. 220.

It is also to be regretted that a telegram was not sent from Melbourne last April circulating the news of Mr. Grigg's discovery of Comet *b* 1907. Mr. Grigg sent a telegram there immediately, so he is not to blame for the delay. It seems advisable to call attention to the erroneous designation of this comet as "Comet Mellish" by Prof. Barnard in *Ast. Nach.* 4199, and by Prof. Aitken in 'Lick Bulletin 120.' Custom would sanction the linking of the names of the two independent discoverers, but to ignore Mr. Grigg altogether, who had a priority of several days, is manifestly unfair to him.

Mr. Grigg has sent an account of his observations on April 8, 10, 11, also those made at Wanganui, N.Z., on April 10, 11, 12, and that at Wellington, N.Z., on April 11 by Mr. G. V. Hudson. All of these anticipate Mr. Mellish's observation on April 13, so there could not be a clearer case of priority.

Mr. Grigg made a careful sketch of the discovery-field and the position of the comet relatively to surrounding stars. I have identified seven of the stars in his sketch as C.P.D. −44° 505, 506, −45° 483, 489, 490, 492, 494. Plotting these, and inserting the comet according to his sketch, I obtain for the discovery-position of the latter:

N.Z. M.T.		App. R.A.	App. Dec.
1907 April 8^d 7^h 28^m	....	4^h 32^m 53^s	44° 38′ S.

the place being referred to the apparent equinox of date.

Mr. Grigg's own observations were made by reading the circles of his equatorial, and comparing with distant Nautical Almanac stars; they are therefore rough, but the comet's motion was so rapid that it is of interest to give them, together with the other New Zealand positions, and Prof. Barnard's photographic position of April 13:—

Date (1907).				App. R.A.			App. Dec.		Observer.	Place.
d	h	m		h	m	s	°	′		
Apr. 8	8	57·9	N.Z. M.T.	4	35	30	44	4 S.	Grigg.	Thames, N.Z.
9	11	53	"	4	59		37	22	"	"
10	7	50·8	"	5	21	8	30	3	"	"
10	9	0	"	5	22	45	29	40	?	Wanganui, N.Z.
11	7	44·5	"	5	42	11	21	38	Grigg.	Thames, N.Z.
11	8	0	"	5	42	40	21	37	?	Wanganui, N.Z.
11	7	45	"	5	42		21	45	Hudson.	Wellington, N.Z.
12	8	0	"	6	0	10	12	58 S.	?	Wanganui, N.Z.
13	14	12·3	G.M.T.	6	27	50·1	0	53·5 N.	Barnard.	Yerkes.

Mr. Grigg says the comet at discovery was 15′ in diameter, and looked like a faint star-cluster involved in nebulosity. Mr. Hudson says:—"It is diffuse, with no defined tail or nucleus. Its substance is extremely attenuated, the faintest stars shining through the mist of it."

On Sept. 12 the Earth passed near the orbit of Daniel's Comet, and, in accordance with a suggestion by Herr Kritzinger, a watch was kept for a possible meteor-shower, but without success. Curiously enough, there is a September shower whose radiant agrees with that of the comet's orbit, but this is probably an accidental agreement.

Prof. Hartwig observed Comet 1907 *c* at Bamberg on June 8, 9, 11. It was 2′ in diameter, without central condensation, fainter than 13th mag. (*Ast. Nach.* 4198).

'Lick Bulletin 120' contains observations of several comets by Prof. Aitken. He followed *a* 1907 (Giacobini) till April 30, and *b* 1907 till May 7, when it was a difficult object even with the 36-inch. He followed Kopff's Comet (1905 IV.) till July 3 last, when it was a very difficult object with no nucleus; he thought it unlikely that it would be seen again. A. C. D. C.

Minor Planet Notes.—Herr Strömberg has computed new elements of 471 Papagena, which will have a very favourable opposition in November, its mag. being 8·5.

New planets have been discovered at Heidelberg as follows:—ZU and ZV on August 8, and ZW on August 29, by Herr Kopff; ZX, ZY by Wolf, Sept. 8; ZZ by Kopff, Sept. 8. ZX, ZY were on plates of the Saturn region, taken for the purpose of locating Phœbe, which appears distinctly on them.

It will be noticed that the double alphabet is now exhausted, and we understand that it has been decided, instead of commencing a triple one, to begin the double one afresh. This will necessitate affixing the year, to distinguish the two sets of double letters.

A. C. D. C.

Obituary.—We regret to hear of the death of Mr. J. G. Petrie, a Fellow of the Royal Astronomical Society since 1889, but who is better known in connection with the British Astronomical Association, of which he had been Secretary since 1895. Mr. Petrie was by profession a journalist, and specially interested himself in city matters. His business capacity always proved a great help to the Association, and at the Council table of that body he will be much missed.

The Total Solar Eclipse of 1908 January 3.—Prof. W. W. Campbell, Director of the Lick Observatory, states that through the generosity of Mr. W. H. Crocker, arrangements have been completed to dispatch an expedition from the Lick Observatory to observe the total solar eclipse of January 3, 1908. The Moon's shadow, in crossing the Central Pacific Ocean from west to east, will pass over only two known islands. One of these is Flint Island, in longitude 151° 48′ W. and latitude 11° 26′ S.,—

that is, about 390 miles north-west of the island of Tahiti. The eclipse will occur at $11^h\ 18^m$ local mean time, with the Sun at zenith-distance 15°. The duration given by the American Ephemeris will be $4^m\ 6^s$.

Also, at the instigation of Prof. Campbell, an expedition has been organized by Prof. Walcott, of the Smithsonian Institution, and Prof. Abbot, Director of the Astrophysical Observatory, for the main purpose of making a bolometric survey of the solar corona. The two expeditions are independent, but will make the journey together, voyaging in an American gunboat from Tahiti to Flint Island.

There is a rumour that an English amateur is taking an expedition to Flint Island.

Cost of Science.—A writer in *Nature* contrasts the amounts spent on scientific investigation respectively by the Governments of Great Britain and of the United States. The total of the former is £242,863, of the latter £2,740,360. The ratio is surprising, and on examination it appears that of the large total spent in America the greater part is due to the Department of Agriculture. The United Kingdom spends on astronomy and the kindred sciences, including the Hydrographic Department, less than £110,000. The similar total in the United States is about £243,000, but of this the Coast and Geodetic Survey spends £198,000. The British Government spends more on astronomical observatories than does the American.

We are asked to announce the following changes in the staff of the U.S. Naval Observatory:—Professor of Mathematics A. N. Skinner, U.S.N., of the U.S. Naval Observatory, was retired according to law upon reaching the age of sixty-two years, on August 12, 1907. Prof. Skinner will remain upon active duty, however, until the completion of some unfinished work on the *Astronomische Gesellschaft* zone −14° to −18°, which was observed under his direction from 1892 to 1894. Mr. H. L. Rice, formerly Assistant Astronomer at the Observatory, has been appointed to the Professorship vacated by this retirement; and Mr. H. R. Morgan succeeds Mr. Rice in the position of Assistant Astronomer. The organization of the work of the Observatory has been changed in the direction of the consolidation of the work; and Prof. H. S. Eichelberger, U.S.N., has been placed in charge of all the astronomical work of the Observatory.

At the annual exhibition of the Royal Photographic Society, now open at the New Gallery in Regent Street, there is an exhibit from the Royal Observatory, Greenwhich, consisting of photographs of Comet 1907 *d*, Saturn and its ninth satellite Phœbe, Jupiter with its sixth and seventh satellites, and of some large sun-spots of the past year.

On Wednesday, September 18, my co-Editor, Mr. Thomas Lewis, Secretary of the Royal Astronomical Society, was married at St. George's, Hanover Square, to Miss Maria Elizabeth Wills, of Cardiff, the bride being given away by Sir William Christie. All our readers will no doubt join me in offering congratulations and wishing happiness to the bride and bridegroom. H. P. Hollis.

This seems to be epidemic. We are informed that another well-known member of the Greenwich staff, Mr. Charles Davidson, to whom we offer congratulations, will enter the marriage state during the present month.

From an Oxford Note-Book.

One advantage of a holiday is that one is impelled to read books which usually get crowded out of the week. Finding myself on a small boat with a yet smaller library, I read *Rasselas* through from beginning to end, a feat not often accomplished, I suppose, in the twentieth century: even though it offers the attractions of instruction without irksome discipline, humour without excessive merriment, and acquaintance with strange races without the need of contemplating strange manners. (Would Johnson pass that sentence, I wonder?) Whether under ordinary conditions I should have reached the "conclusion in which nothing is concluded" is perhaps doubtful: but when the astronomer turned up in chapter xl. it was easy to maintain an interest in his doings, notwithstanding the tantalizing way in which the author evades very definite allusion to his pursuits. On reaching the end of the work, one "conclusion" at any rate suggests itself, namely that Johnson did not take the trouble to learn any astronomy. His friend Goldsmith wrote a book on natural history "more interesting than many a novel"; but Johnson does not seem to have been animated by the same spirit. Perhaps his disinclination towards the Science betrays itself in the utterance of Pekuah:—

> At night the Arab always attended me to a tower set apart for celestial observations, where he endeavoured to teach me the names and courses of the stars. I had no great inclination to this study, but an appearance of attention was necessary to please my instructor, who valued himself for his skill: and, in a little while, I found some employment requisite to beguile the tediousness of time, which was to be passed always amidst the same objects. I was weary of looking in the morning on things from which I had turned away weary in the evening; I therefore was at least willing to observe the stars rather than do nothing, but could not always compose my thoughts, and was very often thinking on Nekayah, when others imagined me contemplating the sky.

Here are two excellent reasons for studying astronomy—the necessity of pleasing one's instructor, and weariness of one's surroundings. But, O Pekuah! didst thou not over-flatter thyself at escaping detection in "sugaring"? Do not all instructors know too well that "appearance of attention" deemed "necessary to please the instructor," and the attitude of those who are "thinking on Nekayah" and imagining that "others imagine them contemplating the sky"? Even Johnson himself when he "withdrew his attention and thought about Tom Thumb" did not claim to have done so undetected.

BUT "the Arab" above mentioned is not "the astronomer" of the book. The latter had an observatory in Cairo and was "one of the most learned astronomers in the world, . . . and had drawn out his soul in endless calculations. He admitted a few friends once a month, to hear his deductions and enjoy his discoveries." He was equal to observing the "emersion of a satellite of Jupiter" when a sudden tempest did not cloud the sky. But his 40 years' studies only resulted in inflicting on him the delusion that he "had possessed for five years the regulation of the weather, and the distribution of the seasons; that the sun had listened to his dictates and passed from tropic to tropic by his direction: . . . . that he had restrained the rage of the dog-star, and mitigated the fervours of the crab." Fortunately he had come to the conclusion that he could not improve on the existing order of things. He had "sometimes turned aside the axis of the earth, and sometimes varied the ecliptic of the sun: but he had found it impossible to make a disposition by which the world might be advantaged: what one region gains, another loses, by an imaginable alteration, even without considering the distant parts of the solar system, with which we are unacquainted."

BUT one thing of value is placed to the credit of the astronomer; he knew how to take a holiday. This fact is not stated boldly, but it is clearly enough written for those who know the indications. This is his actual deposition:—

> When I have been for a few days lost in pleasing dissipation, I am always tempted to think that my enquiries have ended in error, and that I have suffered much and suffered it in vain.

Reading between the lines, this simply means that the good man was able to play so well that not all his work could make him a dull boy. There is indeed no surer sign of rest from engrossing toil than the apparent diminution of all the details which have hitherto seemed so large and important. It is a true effect of distance like that quoted a few pages earlier in *Rasselas*:—

> Distance has the same effect on the mind as on the eye, and while we glide along the stream of time, whatever we leave behind us is always lessening, and that which we approach increasing, in magnitude.

By inexorable law not only the pains but the pleasures and successes of the labour suffer this effect of perspective: and so we get the result produced by the astronomer's "few days of pleasing dissipation." He feels ready to exclaim with Hans Breitmann:

> Vot after all is Earthly bliss?
> Und vot is man's success?
> Und vot is various kinds of things?
> Und vot is happiness?

An expedition to a neighbouring Cathedral city enabled us to supplement our boat's library at a very cheap rate. At a second-hand book shop I was fortunate enough to discover, among vast stores of sermons and other theological literature, a volume of Humboldt's *Cosmos*, and Francis Galton's 'English Men of Science' (1874); the two together costing eightpence. The latter book is a statistical enquiry into the relative importance of "Nature and nurture" in producing Fellows of the Royal Society. This enquiry has recently been considerably extended by the same author, but it was interesting to read the first attempt. There is, moreover, one nice story of an astronomer in it, which is thus presented (pp. 194–5):—

> A strong taste bearing remotely on science may prove very helpful. The love of collecting, which is a trifling tendency in itself, common to children, idiots, and magpies, often leads to the study of the things collected, and is of immense use to a man who wishes to study objects that must be collected in large numbers.
>
> I have been told of an astronomer whose primary taste was a love of polished brass instruments and smooth mechanical movements, that nothing satisfied this taste so fully as work with telescopes, and from loving the instruments he soon learnt to love the work for which they were used.

A recent letter from M. Vallé gives the welcome news that 800 of the Tacubaya plates for the Astrographic Catalogue have already been measured, and only 22 fields out of the 1200 remain to be taken. Considering the unusual difficulties with which M. Vallé has had to contend (the object-glass, for instance, was not found satisfactory, and after some years of vain trials had to be sent for improvement to Alvan Clark), this is a very fine record. Difficulties with the meridian circle have delayed the obtaining of meridian places of the stars to be used for plate-constants; but with provisional places from the Washington zones, equatorial coordinates have been computed for centres 0^h–4^h in zones -15° and -16°, and besides this, 23 charts of the long-exposure plates have been reproduced by heliogravure and issued to other observatories. It is so important to get the measures published that it would almost seem desirable to print them at once, leaving the data for plate constants to follow later.

It is good news (though it may be old news to some) that Professor Hussey is to have a large reflector in his new home at Ann Arbor. He writes, under date September 2, that "after a year's delay Mr. Brashear has received the large disc of glass and has it ready for the optical work. The disc is somewhat over 37½ inches in diameter. The work of building and mounting has begun. During the past two years," he adds, "we have done much constructive work here: the Observatory has been repaired throughout, and we have also had much work done on the instruments, particularly on the 12¼-inch refractor, which has been nearly remounted and made essentially a modern instrument."

From Dr. Schorr, of Hamburg, comes also the welcome news that a new Observatory is being built at Bergedorf. The walls of the buildings are nearly complete, and the domes will be erected next year, when it is hoped that a new meridian circle and a 60-cm. refractor, both by Repsold, will be mounted. In 1909 a reflector of 1 metre aperture and a twin photographic telescope will be added.

SS Cygni.—This star has attracted the keen interest of Mr. C. L. Brook, Mr. A. W. Brown, and other English variable-star observers; and at their request I venture to ask for references to as many published observations of it as possible, with a view to a complete discussion; and also for copies of unpublished observations, if observers will be so kind as to communicate them to the University Observatory, Oxford. If anyone else has already embarked on such a discussion, we should naturally not wish to embarrass his work; but no such undertaking is known to us at present. The star seems to offer better facilities than U Geminorum for probing the mysteries of these semi-regular variations; for the whole light-curve can be obtained without much difficulty, whereas U Geminorum is very faint at minimum.

The following cutting has lost its address, or never had one; and has, moreover, been cut tantalizingly short. One is curious to know what the average ten-year-old boy was going to do. He is, no doubt, a revised version of Macaulay's wonderful "Every Schoolboy."

> happen, but as a matter of fact no one ever does know or can know. An American Presidential election is an algebraic equation with two or three known quantities and some fifty *x*'s to be found. You can eliminate the known factors easily enough—the average ten-year-old boy is

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From the *Times* of Sept. 11 we learn that a thieving porter was caught by the simple expedient of addressing a letter, containing money, to "The Man in the Moon." The porter pocketed the letter, but could not produce satisfactory evidence of his lunar origin.

To His Excellency the Superintendent of —— Observatory :—

[Translation.]

After usual greeting I notify your honour that for two months I have been watching the movement of Mars, and I found that the amount of his movement was about two centimetres a day from East to West, according to fig. A, and that after a month and a half I found it according to fig. B, whereas to-day I find it according to fig. C; and I beg your Excellency to explain to me the reason for its returning back. I want to know, Has the whole universe (*lit.*=the great round) moved towards the west, or what the devil is it? And as I have been polite in my request, I do not think that you can refuse to give me the information; and if any extraordinary things happen in Astronomical matters, or new things, please inform me about it all *concisely*, as it will be useful: and in any case I am grateful, and remain, *your Excellency*, (Signed) ————, Draughtsman.

Cairo, September 4, 1907.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. NOVEMBER, 1907. No. 389.

*Hermann Carl Vogel**.

ONE of the greatest of modern astronomers has passed away. Dr. Vogel is dead. The death of the great German astronomer is one of the most serious losses which has befallen astronomy for a considerable time, for Dr. Vogel stood in the very front rank of present day astronomers.

Hermann Carl Vogel was born on April 3, 1842, in Leipzig, where his father, Dr. Carl Vogel, was a well-known schoolmaster. In 1863 he entered the University of his native town, where he took his degree in 1867. Meanwhile he had become devoted to astronomy, and in 1865, while still a student, he was appointed assistant in the Leipzig Observatory, his interest in the science being due to his acquaintance with Zöllner. His first work at Leipzig was in practical astronomy, but in 1869 he took up the spectroscopic side of the science, assisting Zöllner in his observations of the solar prominences. In 1870 he was appointed director of a private observatory at Bothkamp, in Holstein, where he remained for four years. Into these four years were crowded a quantity of excellent work which at once made the name of Vogel famous in the astronomical world.

In 1874 Vogel published his treatise 'Spectra der Planeten,' in which he summed up his work on the planetary spectra. He suspected the presence of atmospheric lines in the spectrum of Mercury, but in that of Venus he could detect only the slightest difference from the Sun. In Mars he detected distinct lines of aqueous vapour, a conclusion previously reached by Sir William Huggins and confirmed by Dr. Vogel himself many years later. He also examined the spectra of Jupiter, Saturn, and their satellites, and in the spectra of the satellites of Jupiter he noticed strong bands, indicating the existence of atmospheric envelopes. At Bothkamp Vogel made telescopic observations of the planets, especially Venus, while he made visual measures of the radial motions of the stars, the line of research in which he made his greatest discoveries. It soon became apparent, however, that satisfactory and reliable measures could only be made by the aid of photography. In 1874 Dr. Vogel left Bothkamp to accept a position which he had been offered in the new Astrophysical Observatory which the German Government established at Potsdam in that year; and in 1882 he became director of that institution, where he gathered round him a number of observers of great ability, Spörer, Scheiner, Müller, Kempf, and others. In 1883, assisted by Dr. Müller, Dr. Vogel published his first spectroscopic star catalogue, containing details of the spectra of 4051 stars to the seventh magnitude.

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* By Hector Macpherson, Junr., in *Popular Astronomy* for October.

At the Astrophysical Observatory in 1887, Dr. Vogel, assisted by Dr. Scheiner, commenced his photographic measurements of the radial motions of the stars. He photographed the spectra of fifty-one stars bright enough for observations with a 12-inch refractor, and measured the photographs microscopically. The results of the observations were published in 1892. Of these fifty-one stars, the swiftest proved to be Aldebaran, with a motion of recession of about thirty miles a second. This motion proved exceptional, the average speed in the line of sight being ascertained to be ten miles a second.

In the course of these observations, Dr. Vogel made his greatest discovery,—the detection of spectroscopic binary stars. In 1888 and 1889 he tested spectroscopically the theory that the variations of Algol were caused by its eclipse by a dark satellite, and placed it in the region of actual fact. He found that before each minimum Algol was retreating from our system, while on recovering its brightness it gave signs of approach. Working on the assumption that the density of both stars was the same, he reached the following interesting conclusions, that Algol is a globe about one and a half millions of miles in diameter, and that the satellite is about the size of the Sun: the centres of the two stars being separated by about 3,230,000 miles.

In the following year, 1890, a more surprising discovery was announced from Potsdam. By the displacements of the lines in the spectrum of Spica, Dr. Vogel discovered that the bright star was also a spectroscopic binary, both stars revolving round their centre of gravity in about four days. Spica, therefore, is a star similar to Algol, only the plane of the orbit does not lie in our line of sight and it escapes eclipse. These investigations were the direct confirmation of the idea expressed by Bessel in 1844 regarding the "astronomy of the invisible."

Perhaps, however, Vogel's name is best known through his classification of the stars, propounded in 1874 and revised in 1895. The classification is a development of that of Secchi, but Vogel approached the question from another point of view, believing that a rational scheme classification "can only be *arrived at* by proceeding from the standpoint that the phase of development of the particular body is in general mirrored in its spectrum."

In 'A Century's Progress in Astronomy,' the writer has described the classification as follows:—"Vogel divides Secchi's first type into three classes. In the first class, designated as Ia—represented by Sirius and Vega—the metallic lines are very faint and fine and the hydrogen lines conspicuous. In Ib no hydrogen lines are visible, while in Ic the hydrogen lines are bright. This class includes the gaseous stars. In 1895, after the recognition of helium in the stars by his assistant Scheiner, Vogel separated the stars of class Ib from the first type altogether. These stars are sometimes designated as type 0, and sometimes as helium stars and Orion stars, as the majority of the stars in Orion are of that type. The solar type is divided into two classes, IIa being represented by the Sun, Capella, and other well-known stars, while IIb includes the Wolf-Rayet stars. Secchi's third and fourth types are both classified by Vogel as of the third type."

In Dr. Vogel's opinion, Orion and Sirian stars are the youngest orbs. He believed that solar stars have wasted much of their store of radiation, while, in his own words, red stars are "effete suns, hastening rapidly down the road to final extinction." Stars of his class IIIb, or Secchi's fourth type, are, in his opinion, also dying stars, both types representing alternative roads for stars of the solar type in their decline. This view is supported by Professor Dunér and Professor Hale. Dr. Vogel's classification and scheme of evolution is generally accepted, and it will long be remembered as a masterly effort to unravel the secrets of the universe. His scheme of evolution is distinctly supplementary to the nebular theory.

Dr. Vogel's eminence as an astronomer was recognized all over the scientific world. Member of many scientific societies, he received the Valz Prize of the Paris Academy of Sciences in 1891, and the Gold Medal of the Royal Astronomical Society of London in 1893. In his native country, too, his services to science were fully recognized. In 1895 he received the Prussian Order of Merit, and in 1899 he became Privy Councillor of Germany.

In this article we have mentioned only the chief investigations and discoveries in Vogel's busy life—a life of ceaseless activity. His devotion to astronomy increased, rather than diminished, with the years. In 1905, in a letter to the writer he mentioned that he was at that time "overburdened with work." Some time ago his health began to fail. A malady which afflicted him about seven years ago recurred in the present year, and in the latter end of July he grew gradually worse. He died at Potsdam on August 14, 1907, in his sixty-sixth year, and the funeral service of one who had done so much for astronomy, and who had been the shining light of the Observatory at Potsdam, was fittingly held in the dome of the great telescope with which he worked so skilfully and so well. His death leaves a blank in astronomy *which cannot well be filled.*

Some Counsels of Perfection *.

Even in Astronomy it is hard to bring about an improvement, however obvious the benefit may be, if it involves a departure from time-honoured conditions.

At the beginning of last century the R.A.s of stars were taken in time, laboriously converted into arc for publication, and reconverted into time for use. Again, at that epoch signs of 30 degrees encumbered the tables. The writer does not know who we have to thank for the printing of R.A.s in time and the omission of the Signs of the Zodiac—but we thank him.

It is strange that one set of angles should be measured in time and another set in arc. A revolutionist would propose time for all angles—that is, that the quadrant be taken as six hours. But such a change is too radical to be adopted. The greatest part of the inertia which resists this change is due to the logarithmic tables in common use, which consider only the division into degrees, minutes, and seconds of arc. It is true that both Shortrede and Herz have provided excellent "time"-tables, but they are not in common use. It seems, for measurements along the equator, longitudes, and right ascensions, that time divisions should always be used, but for other angular measurements degrees—that is to say, degrees omitting minutes and seconds. The epoch has now arrived when the degree should be decimalized. The advantages in printing, writing, and brevity are great. Thus, to indicate a quantity to one second of arc we write, say, $1^\circ\ 12'\ 13''$, or five figures and four symbols, and our accuracy is 1/3600th of a degree. If we use decimally five figures and two symbols, such as $1^\circ{\cdot}2234$, our accuracy is about three times as great, with an actual saving in writing, printing, and space.

Our present common 7-figure logarithmic trigonometrical tables are $10''$ tables. Tables to $1''$ are rare and rather large. Decimal tables to $0^\circ{\cdot}001$ would be a happy medium, as $0^\circ{\cdot}001$ corresponds to $3''{\cdot}6$. A thousandth of a degree, 0·001, might be called a grade and indicated by $'$, as the centesimal division of the quadrant is now quite obsolete.

The difference of log tan at 6° is 2025 for $10''$—rather large; this would become 729. Under 4° the tables should go to $0^\circ{\cdot}0001$, then no difference would much exceed 1000, and, all over, the average difference would be about 200.

It is curious how the metric decimal system has failed to take on and how it is being replaced by the metric kilo system. Thus we have a mille-gram, a gram, and 1000 grams equal to a kilo, and 1000 kilos equal to a ton. In measure, the micron, millemetre, metre, and kilometre, and so on, showing how the natural multiples and subdivisions go by thousands.

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* [This note has been sent us by a well-known professional astronomer, who wishes his name not to be affixed.—Eds.]

An almanac for a year takes as its starting-point the beginning of the year instead of the mean or middle of the year; therefore reductions to the end of the year involve the factor $t = 1$, whereas the mean of the year would reduce this factor to $\frac{1}{2}$, and for t^2 to $\frac{1}{4}$. Those engaged in the observations of polar stars and reductions of comets' places, &c., are aware that whilst $t^2 = \frac{1}{4}$ is often negligible, $t^2 = 0{\cdot}97$ or some other number near unity is not so.

Another little difficulty is our unwieldy calendar of months and years of unequal length. The variable-star investigator rejects both, and works by Julian days, and this tendency is also evident in minor planet work, where the mean motion is always given per day (which is definite) instead of per year (which is not). Would it not be better to adopt as the unit of time the solar day, and for quantities too small for one day multiples by 1000's? In carrying forward the elements of a planet from one epoch to another, or the precise place of a polar star, one is driven to make use of the number of days elapsed, so that the variations given have to be reduced to the day as unit. If we had definitively adopted the day as unit of time, what a mass of discussion over Stone's unit of time-change would have been saved, as well as the little error first clearly indicated by him in our present count of time.

In the 'Nautical Almanac' (excellent and invaluable work) all the months have an extra day; thus January has 32 days, and so on. Why 32nd January? Would not February 1st be a little bit better, or might not the last date be omitted altogether, those figures which will be useful in interpolation being left?

The Sun as a Twinkling Star *.

With the employment of a suitable screen of some woven fabric (such as canvas or linen) to shield the eye from a too-blinding glare of sunlight, direct naked-eye observations of the Sun may be made which show that the Sun, shining in a cloudless sky, presents the normal appearance of a twinkling star, only on a greatly enlarged scale. Attentive observation of the phenomena of "twinkling," associated by this means with the Sun, confirms the conclusion that, in their origin, they are to be traced to the same causes exactly as provoke the normal scintillations of the stars.

But it is to be noted that certain characteristics of these phenomena are most readily and reliably to be observed only in association with observation of the Sun, owing, doubtless, to the comparatively great diameter of the area of illumination that offers for the purposes of the observation.

Thus, the Sun offers special advantages for study of the various characteristics, and the possible sources of the interferences, that affect the direct passage of light through the Earth's atmosphere.

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* By Miss C. O. Stevens, in Symons's 'Meteorological Magazine' for September.

In the case of the Sun, the phenomena of "twinkling" resolve themselves into the sweeping across the line of sight of bands or waves of alternately greater or less illumination, in parallel formation, lying at right angles to the direction of their propagation, and observation has shown, that while their direction of movement is variable, it bears a constant relation to the directions of drift of the atmosphere prevailing at the time and place of observation, whether these contemporaneous conditions be determined by ordinary methods (wind-vanes, kites, &c.), or whether they may *only* be determined by the more searching method of telescopic observation of the features of atmospheric distortion of the Sun's limb.

In offering a solution of the phenomena of stellar scintillations, Montigny surmises the occurrence in the upper regions of the atmosphere of the passage, across the line of sight between the star and the observer, of transparent prisms of air. The physical reality of such prisms of clear air is, presumably, demonstrated in the photographic records of the Sun, taken at the Royal Observatory, Greenwich, and elsewhere, which appear to testify to a waved, or billowed, configuration of the atmospheric strata, and even to testify to the variability of their formation in detail, as well as of their direction and rate of movement. In other words, the features of atmospheric distortion of the Sun's limb vary concordantly with variations in the direction and force of drift of the overlying strata of the atmosphere at the place of observation.

Further confirmation of the same is supplied by recorded observations of "shadow bands" at times of total solar eclipse; and also in the familiar features of "rippled" cloud structures. It is again, and perhaps pre-eminently, confirmed by the fact that (in spite of their visibility to the naked eye being theoretically denied) stellar scintillations are easily to be traced in the zenith, though at all times less easily here than at lower altitudes.

The direction of propagation of the "twinkling" waves is a delicate element to determine precisely by naked-eye observation of the Sun, and requires, on the part of the observer, an absolutely fixed point of view, such as is only to be secured by the use of some kind of head-rest.

An Eclipse in the Reign of Nero.

In Tacitus, 'Annals,' xiv. c. 12, we read (Church & Brodribb's translation):—"Then the Sun was suddenly darkened and the fourteen districts of the city were struck by lightning." The date is the year of the murder of Messalina, recently put on the stage by Mr. Tree at His Majesty's Theatre. Oppolzer's diagrams show *that* no eclipse occurred anywhere near Rome for ten years before or after the death of Messalina, except one on 59 April 30. As this happens to be the actual year, there is strong reason for connecting the darkening of the Sun with this eclipse, and the context connects the occurrence with Rome more definitely than by the mere fact that Tacitus lived at Rome and was a Roman writing for Romans.

Unfortunately the central line passed five degrees or more away from Rome, and no conceivable alteration of tables will make the eclipse central at Rome. The interest centres, therefore, upon the excellent example this eclipse affords of the Megalosaros, a period of 1805 years and a few days, which enables the central line of this eclipse to be drawn with ease with the aid of the 'Nautical Almanac' diagram for 1864 May 5.

M. Oppert first drew attention to the fact that 1805 years is a good eclipse cycle, and Mr. Crommelin suggested the name Megalosaros in *Knowledge*, 1903.

The period defined as containing 22,325 lunations also contains nearly 24,227 draconitic months and nearly 23,926 anomalistic months, and it is intermediate between 1805 tropical and anomalistic years. The secular changes in these various kinds of month and year cause the precise numerical relations to change.

The process of making use of the cycle is as follows (it depends, necessarily, upon a carefully drawn diagram for the later eclipse being available):—First, select the formulæ to be used for the various arguments. Secondly, calculate the interval in which mean conjunction in longitude exactly recurs. Thirdly, calculate the interval in which true conjunction in longitude exactly recurs. The difference between these two intervals depends mainly upon the change in the equation of centre of the Sun, and amounts to nearly two hours at the equinox. Fourthly, calculate the change in the Moon's latitude in the interval between true conjunctions. Finally, the interval between true conjunctions gives the displacement in terrestrial longitude between one central line and the other, and the change of latitude, which is small, has to be allowed for.

P. H. Cowell.

The Sun-spots of 1907 *July.*

The magnificent group of spots, Group 6205, that made its first appearance in June, returned to the visible hemisphere, as Group 6215, during July; but it was now greatly diminished from its former extent, and the chief disturbance of the month was Group 6216, a fresh outburst a little to the north of Group 6215. Both groups were visible to the naked eye when near the centre of the disc.

There were not many other groups calling for special notice, the two chief being Group 6211 at the beginning of the month and

Group 6220 at its end. The remaining groups were neither large, numerous, nor specially interesting, but the Sun was never entirely free from spots.

Rotation 720 began July 21^{d}·836.

Spot-Groups in 1907 July.

No. of Group.	Observed.		Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	From	To					
	1907.	1907.	days.	d	°	°	
6210	July 2	July 3	2	June 30·0	289	+12	Very small spot.
1	2	12	11	July 7·1	194	+ 4	Irregular stream.
2	3	6	4	8·0	183	− 8	Small spot.
3	10	15	6	11·1	142	+ 8	Pair of clusters.
4	11	17	7	12·7	121	+13	Small spot.
5	11	23	13	17·0	64	−19	**Return.** Large, regular.
6	11	23	13	17·8	54	− 6	Very fine stream.
7	12	12	1	16·7	68	−14	Pair of small spots.
8	19	25	7	19·5	31	−20	Small stream.
9	20	21	2	16·0	77	−15	Small, faint spot.
6220	20	31	12	26·2	303	− 7	Large composite spot.
1	23	24	2	21·8	1	+11	Two small spots.
2	23	24	2	26·4	300	− 3	Pair of spots.
3	24	Aug. 4	12	29·9	253	+ 8	Straggling stream.
4	25	July 25	1	26·7	296	− 8	Small spot.
5	27	Aug. 5	10	Aug. 2·8	202	+10	Double spot.
6	28	8	12	3·0	199	− 9	Small regular spot.
7	29	5	8	4·1	184	−10	Small regular spot.

Notes on the Principal Spot-Groups.

Group 6211.—A fine but somewhat irregular and unstable stream. Area on July 6 about 300.

Group 6213.—A few small spots, gradually developing into a pair of clusters. Area on July 13 about 180.

Group 6215.—Return of Group 6205. A very large, well-defined, regular spot, with occasionally one or two very small companions. Area on July 17 about 700.

Group 6216.—A very fine, irregular stream, undergoing continual change. Area on July 16 about 1500.

Group 6220.—A large composite spot, sometimes appearing as a large circular spot with very close companions. Area on July 26 about 350.

Group 6223.—An irregular and unstable stream, diminishing quickly after July 26. Area on July 26 about 180.

E. Walter Maunder.

The Great Red Spot on Jupiter.

The variable motion of this object in recent years formed a rather difficult problem, but it was thought that the solution had been found in the accelerating influence exerted upon it by the great S. temperate spot. The latter moves about 22 seconds quicker (in one rotation period) and is in same latitude as the red spot. It has been found that, whenever the two markings mentioned come into conjunction (as they do once in about every 700 days), the motion of the red spot is strikingly quickened and becomes displaced to the westward, the period of its rotation and the longitude of its position becoming less.

Thus its longitude, computed on the basis of a period of $9^{h}\ 55^{m}\ 40^{s}{\cdot}6$, was as follows on the dates given :—

1905 March 27	24°·6
1905 June 24	25 ·1
1906 May 4	32 ·2
1906 August 8	16 ·3
*1907 April 18	26 ·4
*1907 September 24	18 ·3

Rotation Periods.

	h	m	s
1906 June to May	9	55	41·5
1906 May to August	9	55	34
1906–7 August to April	9	55	42
1907 April to September	9	55	39

The acceleration of the velocity to the extent of 7½ seconds in 1906 occurred during the very period that the S. temp. spot was in conjunction with the red spot and apparently communicating to it a portion of its swifter velocity. Thus the shorter rotation-period and decrease in longitude can be readily explained. But the acceleration during the past summer months requires an essentially different theory, for the S. temp. spot was on the opposite side of the disc to the red spot. Is it possible that there is a minor acceleration at about the time of opposition of the two objects? This is an interesting point, only to be satisfactorily determined by further observations. The S. temp. spot has been visible since February 1901, and seems likely to continue in view for many years. Jupiter's altitude when southing is about 56 degrees from the latitude of Greenwich, so that the study of his surface-markings may be conducted under good definition and with every prospect of success at the present time.

The conjunctions of the red spot and S. temp. spot have occurred or will occur on about following dates :—

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* For observations in 1907 I am indebted to Mr. A. Stanley Williams and the Rev. T. E. R. Phillips, as I have been unable to use a telescope this year.

1902 July.
1904 June.
1906 May.
1908 April–May.

Months of opposition are, of course, intermediate with those in the list.

Bristol, 1907, Oct. 15. W. F. DENNING.

Occultations of Neptune by the Moon visible at Greenwich in 1907.

THE following occultations of Neptune, visible at Greenwich, were accidentally omitted from the *Nautical Almanac* for 1907 :—

Date.	Disappearance.				Reappearance.			
	Sidereal Time.	Mean Time.	Angle from		Sidereal Time.	Mean Time.	Angle from	
			N. Point.	Vertex.			N. Point.	Vertex.
1907.	h m	h m	°	°	h m	h m	°	°
Nov. 23	1 4	8 57	131	171	1 47	9 40	218	260
Dec. 20	11 14	17 20	180	138	11 23	17 29	195	153

H.M. Nautical Almanac Office,
1907, Oct. 15.

A. M. W. DOWNING.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Fomalhaut.

GENTLEMEN,—

This star is of rather special interest in this country, as being the southernmost first-magnitude star which is visible in our skies, its meridian altitude at Greenwich being little more than eight degrees. The only meteorological forecast I ever remember hearing Sir George Airy indulge in was the remark, when told one evening that Fomalhaut was visible, that it would be a fine night. The origin of the name seems difficult to trace. The first mention

I can find of it is in the Alfonsine Tables [*], where (as Ideler remarks) it is erroneously spelt Fomahant. It is there placed amongst the stars of Aquarius, who in old schemes is represented as pouring a stream of water into the mouth of the fish.

Whence Alfonso obtained the name Fomahant, or its more correct equivalent Fomalhaut, I do not know. That the word is compounded of two Arabic words, and signifies the mouth of the fish, there is no doubt. But the Arabs called the star the frog, or rather the first frog, the second being β Ceti, which follows it by nearly two hours of right ascension. Fomalhaut, in fact, they call *El-difda el awel* (the first frog), β Ceti, *El-difda el thani* (the second frog). Aratus puts them together without names, saying that near the feet of Aquarius, where the flow of water falls (as translated by Mr. Robert Brown),

> More clearly 'mid them move a pair of orbs,
> Nor very far away nor very near—
> One large and bright beneath the *Pourer's* feet,
> The other 'neath the dusky *Monster's* tail;
> And all are called the *Water*.

The "dusky *Monster*" is of course Cetus, which was afterwards called Κῆτος (Cetus), and sometimes 'Ορφὸς. Ptolemy places Fomalhaut (without name) both in Aquarius and Pisces Notius, describing it in the former place as ὁ ἔσχατος τοῦ ὕδατος καὶ ἐπὶ τοῦ στόματος τοῦ νοτίου ἰχθύος, in the latter as ὁ ἐν τῶ στόματι ὁ αὐτὸς τῆς ἀρχῆς τοῦ ὕδατος; so that it was the end of the flow of water in the former and the beginning of it in the latter, the fact of the supposed *fall* making no difference as to the direction.

It seems a pity that Piscis Australis should not rather have retained the adjective Notius (as Bode does in his Atlas) or Austrinus (as in Flamsteed). For the zodiacal constellation Pisces is double, and the two fishes are not close together, like the Twins in Gemini, but at some distance, connected by a band or ribbon, and one of them was sometimes called the southern fish. Ptolemy, however, calls this the preceding or western, and the more northern the following or eastern fish. The Scholiast on Aratus says that the northern was represented with the head of a swallow, and was therefore called χελιδονίας ἰχθύς, supposed to be the tunny-fish.

Fomalhaut has a considerable, but not large, proper motion. Its parallax is one of those which were determined with the Cape heliometer, under the direction of Sir David Gill, and found to amount to 0″·15.

Yours faithfully,
W. T. Lynn.

Blackheath, 1907, Oct. 3.

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[*] Not many names of stars are mentioned. Stella Polaris is also called Alrucaba, *a* Coronæ Alpheta, *a* Herculis Rasalheti, *a* Cassiopeiæ Scheder, and *a* Persei Alchemb. Regulus appears as Rex.

The Loss of Energy by the Sun.

GENTLEMEN,—

I am far from thinking that I can give a satisfactory answer to all Prof. Newcomb's objections, but I think I can somewhat lessen their force. As to the loss of heat by the Earth on a clear night, I may remark that millions of meteors enter the atmosphere every night, and that a very much greater number must be near enough to have their temperature considerably affected by terrestrial heat. And there is reason to believe (without going the length of Lockyer's theory) that considerable numbers of them are scattered through space at all distances from us. Then we already know of more than 500 asteroids, and how many more exist we can only conjecture. Great numbers may exist unperceived in the remoter parts of the solar system. The amount of heat which they absorb would be much greater than their mass would account for, since the smaller a body is the larger will be its surface as compared with its mass. They are colder than the Earth, not only because of their greater distance from the Sun, but because our atmosphere helps to keep up our temperature, and we still retain a good deal of our original internal heat. I would therefore ascribe the loss of terrestrial heat on a clear night chiefly to dark objects situated within the limits of the solar system, which we now know to be much more numerous than was supposed sixty or seventy years ago, and are still imperfectly ascertained.

I fear I have not sufficiently studied the laws which govern the propagation of waves in elastic fluids to deal satisfactorily with his next objection; but his language seems to me to carry the analogy between emission and the propagation of vibrations too far. Vibrations in a fluid can bend round an angle to a certain extent, as in the case of sound, and I believe light can also bend round an angle, though to a very slight extent. A very slight deviation from rectilinear motion would suffice to allow the ray to reach us from the apparent instead of the true direction of the Sun; but whether this would conflict with the aberration of light or afford an explanation of that phenomenon I must leave others to consider.

I may, however, mention a third alternative, which I have elsewhere suggested, viz., that the ether absorbs heat and light, which consequently is not lost but absorbed, *i. e.* transformed. Ether transformed in this manner would no doubt acquire new properties, and perhaps it might become a very attenuated nebula. And it may be that when a nebula previously unobserved seems to spring up around the great outburst of light and heat involved in a new star, it is composed of ether transformed by absorption of heat. And besides this hypothesis, and that on which Prof. Newcomb is commenting, there is a third, which would avoid the necessity of supposing a continual loss of heat or energy throughout the Universe, viz. that dark bodies (especially the small ones) were so numerous throughout space that a ray emitted from any celestial body could not fail ultimately to encounter one of them and be absorbed by it. Any of the three seem to me more probable than that unabsorbed heat is constantly leaving the Universe (as known to us) and never returning. Truly yours,

Dublin, 1907, Oct. 10. W. H. S. MONCK.

The Transit of Mercury.

GENTLEMEN,—

On the occasion of the forthcoming transit of Mercury perhaps some astronomer will interest himself in looking for the suspected atmosphere of the planet. Not the so-called "halo," but a luminous ring seen when Mercury is *outside* the Sun's disc at ingress or egress. Such a ring was seen with certainty surrounding Venus at the two last transits, but for Mercury the evidence is not yet conclusive. There are observations of it by Plantade at Montpelier in 1736, and by Simms in 1832, referred to in the late Miss Clerke's 'History of Astronomy' (3rd ed. p. 302). I wrote a note on this subject in 'Bulletin de la Société Astronomique de France' (1902, p. 37), but documentary evidence is very meagre. The next transit will afford a good opportunity for visual and spectroscopic search, which I hope will be made.

I am, Gentlemen,
Yours faithfully,
M. MOYE.

University of Montpellier, France,
1907, Oct. 9.

"Our Debt to Astronomy."

GENTLEMEN,—

May I be permitted to traverse a statement which occurs in Prof. R. T. Crawford's address under the above title printed in the last number of the *Observatory*? I do so in the interests of historical accuracy. The statement reads as follows:—"The Church had taught that the Earth was stationary, and even to think anything else was the greatest heresy, as it was contrary to Holy Writ." It was never a dogma of the Church that the Earth was stationary, and consequently to think the contrary was not only not the greatest heresy, but no heresy at all. The dogmatic teaching of the Church can suffer no change with the advance of scientific learning; the interpretation of non-dogmatic passages of Scripture, of which the Church is the guardian, can be changed. The following extract from a letter of Cardinal Bellarmine, one of Galileo's judges, to the Carmelite Foscarini, under date April 12, 1615, illustrates this point:—"If a true demonstration should be found that the Sun is placed at the centre of the world, and the Earth in the third heaven, and that the Sun does not turn round the Earth, but the latter round the former, then it will be necessary to proceed with great prudence in the explanation of

Scripture, which seems to say the contrary, and rather to avow that we have not understood it, than to declare a demonstrated fact false." *Ne sutor ultra crepidam.* Had the illustrious Galileo been content with his astronomical discoveries, and not attempted to play the amateur exegetist, he would never have been condemned.

Prof. Crawford proceeds:—" In view of this, is it any wonder that we had the Dark Ages ? " My reading of history is that the Dark Ages were due to the incursions of barbarians, who swept away the church and monastic schools, which were the centres of culture and learning for laymen and churchmen alike. At any rate, astronomy constituted, with arithmetic, geometry, and music, the *quadrivium*, or subjects taught in the schools for higher studies; and the culture of that illustrious scholar Gebert, afterwards Pope Sylvester II., even in matters astronomical, who flourished in the darkest of the Dark Ages, would do credit to any age.

Yours faithfully,
A. L. Cortie, S.J.

Meteoric Showers in Aquila, Boötes, and Pisces.

Gentlemen,—

On Sept. 11, $9^h\ 23^m$, I saw a very interesting meteor between mag. 1 and 2, moving with extreme slowness upwards from $287\frac{1}{2}° +22°$ to $282\frac{1}{2}° +31°$. The radiant-point was evidently close to the variable star η Aquilæ. The meteor faded almost to invisibility in the middle part of its course, but burst out at the end with increased brightness and left a momentary trail of yellow sparks. The radiant is one of long duration, for I have recorded meteors directed from it between February 20 and Nov. 7 in various years. It appears to be specially active in April, May, July, and August. There is a shower which appears to be of the same length of duration from near ζ Boötis at $220° +13°$. Meteors have been seen from this position between Dec. 5 and Aug. 25, but the activity of the radiant is most strongly marked in January, when the meteors move very swiftly and leave streaks.

I may mention another shower at $346° +1°$ near β Piscium, which apparently continues active from June until December. It is usually best displayed in Aug. and Sept., and I have traced a large number of meteors, including many bright ones, from it during the last thirty years. This particular radiant agrees in position with the radiant computed for Daniel's Comet on Sept. 12 by Kritzinger (A. N. 4199). I believe, however, the radiant has never been observed to exhibit special activity, though it has yielded several fireballs, at about the middle of September.

The coincidence in position between the radiant of β Piscids and the radiant of Daniel's Comet is probably accidental. There are such a large number of meteoric showers known that the calculated radiant-point of any comet must agree with one of them approximately, and such resemblances of position are naturally suggestive, though they often mean nothing more than mere chance agreements in place.

I maintained short watches of the sky on the nights of Sept. 10, 11, 12, 14, and 16, in the hope of seeing a few meteors from Daniel's Comet, but failed to notice any sign of the β Piscid radiant, though there was a well-defined shower from 355° +5°.

Yours faithfully,

Bishopston, Bristol,
1907, Sept. 12.

W. F. Denning.

Mr. Tebbutt and the Royal Society's List of Scientific Papers.

Gentlemen,—

We all have to make such frequent reference to this list that it may be worth while to call attention to the fact (although it is mentioned as an erratum in vol. xi. p. 559) that in vol. viii. p. 1065, seven of Mr. Tebbutt's papers are placed in a separate category and ascribed to "John Tebbutt, jun.," as if he had been a different person from "John Tebbutt," to whom the rest (beginning with the first of all, on Comet II. 1862, *Ast. Nach.* lix. col. 183, in vol. v. p. 927) are assigned. Mr. Tebbutt informs me that his father (who did *not* make astronomical observations) died in December 1870, after which he, of course, dropped the "Junior."

Perhaps it may be of interest here to refer also to the fact that Mr. Tebbutt's first astronomical achievement was the discovery of the splendid comet of 1861, which he was the first to notice on the 13th of May, and communicated to Mr. Scott, then Director of the Sydney Observatory, who did not succeed in obtaining an observation until the 27th, when the comet was just visible after sunset to the naked eye (*Monthly Notices*, vol. xxi. p. 254). It was seen both at Hobart and at Williamstown on the 4th of June, Mr. White's observations at the latter place commencing on the 6th. It passed its perihelion on the 11th of that month, and entered the northern hemisphere on the 16th.

This comet was 1861 II.; Mr. Tebbutt also discovered Comet 1881 III., which became just visible to the naked eye in June.

Yours faithfully,

Blackheath, 1907, Oct. 16.

W. T. Lynn.

OBSERVATORIES.

The Royal Observatory of Belgium.—A recent visit to this comparatively new observatory at Uccle, near Brussels, showed that, under the energetic direction of M. Lecointe, nothing is being spared in order to equip it in the best possible manner.

It has already been noticed in these pages that this observatory has taken over the work of making the Astrographic Chart plates originally allotted to Potsdam. At the time of our visit the photographic telescope had just been mounted, and although not complete in all details, experimental work had already been begun. The telescope, carried on an English mounting, is similar to the Paris astrographic telescope. The very spacious dome is provided with a rising floor, which adds greatly to the comfort of the observer, and the dark room, situated in the same building, is a large apartment fitted up with every convenience for use as a photographic laboratory. It is understood that the reproductions of the Chart plates will be done in the observatory, and that they will be photogravures similar to those sent out by the French observatories.

Great precautions have been taken to ensure an even temperature both in the dark room and in the dome; and this remark applies also to the other observing-rooms throughout the observatory, where an even temperature is maintained by means of electric ventilating fans.

The Reversible Transit-Circle by Repsold has an aperture of 16 cm., and appears to be a very fine instrument. Another meridian instrument (the Gambey Transit, of 16·7 cm. aperture) is also in use, and, in connection with these two instruments, azimuth-marks have been erected, two about 175 metres to the south, and two some 65 metres to the north. In the erection of these great care has been taken to guard against variations of temperature. Special buildings have been erected for this purpose, and, following on the plan adopted at Paris, long tubes carried on iron trestles connect the marks with the observing-room.

The Observatory is responsible for the provision of Greenwich Time throughout the country, and a splendid installation of clocks has been made. The Standard Clocks—four in number, two Sidereal and two Mean Time—are set up in an inner chamber of an underground cellar specially built for the purpose, where the temperature is kept uniform. Two of these clocks act as reserves, the mere changing over of a switch being all that is necessary in the case of failure on the part of one of the clocks actually in use.

The clocks of the Observatory are synchronized by these Standard Clocks. This is done by means of an electro-magnet placed so as to act upon the pendulum of the synchronized clock. At every alternate beat the Standard Clock closes a circuit and a current is made to pass through the electromagnet, which in turn influences the swing of the pendulum of the synchronized clock.

A regular series of observations of double stars is being made with the Cooke Equatorial of 38 cm. aperture. And we should like to acknowledge here the kindness of M. van Biesbrœck, as well as that of the members of the Staff whom we met, in showing us over this interesting institution. Observations of sun-spots are being made daily, and a spectroscopic department is in process of formation.

The Observatory is placed in a fine, commanding position on the outskirts of Brussels, and is easily reached by tram. In order to protect it from outside interference, the surrounding land has been bought by the Government, and houses for the accommodation of the staff will be erected on this. P. MELOTTE.

MELBOURNE.—The report of this Observatory for 1906 is very much like those of previous years. The Board of Visitors ask the Government for money for publication, and also to fill the office of Chief Assistant. The principal, in fact the only astronomical, work now being done is the Astrographic Catalogue and Chart, and it appears from the report that about half the Catalogue plates taken at Sydney have been measured, and about three-quarters of those taken at Melbourne. A duplicate series of Catalogue plates is being taken, and the Chart plates of the zones of odd declination have been taken with three exposures. Those at even degrees of declination have been taken with exposures of one hour, and it is proposed to re-take these, giving three exposures of half an hour. This is, indeed, doing things generously.

At a Conference held at Adelaide in 1905 May, to discuss certain matters in connection with the observatories of Australia, it was resolved that it was not expedient to separate astronomy from meteorology. Notwithstanding this, an Act was passed in 1906, in virtue of which the Commonwealth Government is to take over the State weather services, to form a single homogeneous weather organization under Federal control, leaving the astronomical institutions to the care of the respective States as hitherto, which last is quite in accord with the recommendations of the Board of Visitors. The Melbourne Observatory will therefore be relieved of all its meteorological duties by the end of the year 1907.

PUBLICATIONS.

A HISTORY OF ASTRONOMY*.—The announcement of the appearance of a book entitled 'A History of Astronomy' not unnaturally suggests the question whether there is in our shelves a gap which this book can be expected to fill. *A priori* we should answer in the negative; *a posteriori* we frankly modify that answer into the advice to make a gap for it. We do not for a moment suggest that Mr. Bryant's book should displace Delambre or Grant nor even Miss Clerke; those are in a different category. Perhaps Mr. Berry's 'History of Astronomy' enters more nearly into competition with the book before us; but there is a clearly marked divergence in the aims of the two books, inasmuch as the former has compiled a book of reference for students, while Mr. Bryant has aimed at a lightly written history for the delectation of amateurs.

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* By W. W. Bryant, B.A., F.R.A.S., F.R.Met.Soc., of the Royal Observatory, Greenwich. Methuen & Co. Price 7*s.* 6*d.*

And to the amateur the book can be cordially recommended. He cannot fail to feel that he is listening to a man who is in possession of full and practical experience of his subject. And perhaps it is just the practical details which will be found the most interesting, such as the chapter on Adjustment of Instruments, in which some account is given of the inevitable difficulties connected with personality, or in such *obiter dicta* as the following:—"Herschel was probably the first astronomer who considered his eye as part of the observing instrument, and was careful to adjust the position of his head in order to view such an object as a band on a planet in the same direction relative to the position of the retina." We hear so much about the science of astronomy that these references to the art are refreshing.

The illustrations naturally catch the attention as one opens the book; the selection is made with discrimination; the one or two old friends are welcome, while the four from Hevelius are well worth reproduction. It seems a pity to have omitted that one of Hevelius' comets of which the tail is discontinuous; such an anticipation of Barnard's photographic discovery of shattered tails deserves attention. A word of praise is due to the careful account given of the origin of each illustration; the "Newton" by Kneller must surely be well-known. We believe it is now hanging at Petworth. It is annoying, however, to find the photographs of nebulæ indiscriminate in their orientation. The Pleiades have the North at the top, the Ring Nebula has the West at the top, and the rest have the South uppermost.

The style of the whole book is light and pleasant and induces one to forgive an occasional obscure passage. Sometimes, however, the obscurity is such as to baffle all attempts at comprehension. There is a sentence on p. 163 in which "the axial rotation effect" in the Sun is apparently made to account for "well-marked frequency zones for spots," and another on p. 31 containing reference to "the practically infinite distance of the point to which the Earth's axis is directed." We confess that these defeated us.

Perhaps the greatest difficulty that the critic finds in estimating the success of the book consists in arriving at any idea as to the class of audience that is being addressed. On p. 15 the obliquity of the ecliptic is carefully explained, while on p. 59 an argument such as $(5S-2J)$ is lightly referred to and seems to call for no enlightening footnote. One cannot help sympathising with an author who is full of information; it costs a pang to omit anything, but there can be no question that there is too much in this book. With p. 125 open before us we find that there might have been eleven footnotes, all giving reference to first-class papers. Such a page cannot leave any useful impression on the mind. Difficult subjects like the interpretation of spectra are treated so sketchily that the account can scarcely be useful to the unenlightened, while it is superfluous for others.

The biographical part, covering the first 96 pages, suffers least from the congestion which mars so many chapters. Surely Copernicus did think that the stars were all the same distance from the Earth; and ought not a special reference to be made to Tycho's initiation of the important practice of repeating observations? "Tycho next married a plebeian girl on his own account," which is called a "descensus Averno." In passing we cannot omit to note with satisfaction that both in the case of "Averno" and of the "smaller fleas" later in the book, common misquotations have been avoided. Hevelius should have the credit of the optical libration, while Galileo claims the diurnal. Can Mercury have been seen in transit before Kepler's time, that is without a telescope? We must test this on the 14 inst.

Three chapters on the Sun are full to overflowing, but the interest is well maintained in spite of several exceedingly cryptic passages.

In the five chapters on the Stars the impossible task has been attempted of covering the whole ground from Fundamental Astronomy to Spectroscopy in 70 pages. But when once we have asserted the impossibility, we must admit that the reader finds it difficult to put down the book, the matter is so clearly sound, and the manner is cautious and philosophical. There is a pleasant absence of dogmatic statement; in fact the fault is rather in the opposite direction. Such phrases as "it is hinted that" might often be replaced by a more direct assertion. We are, for instance, told that it is doubtful whether the lunar rills are or are not dried-up water-courses, when the negative has been asserted by more than one competent authority. Dr. Rambaut has done more than "hint" that he can produce the green flash before sunset. Again, we read that the case stated by Kapteyn and Eddington might be "met by assuming for our system a position well outside the centre of a single universe, so that a fair proportion of its members by virtue of their position, like planets at inferior conjunction, would appear to be moving in a different direction to the rest." This suggestion has been disposed of by Mr. Eddington as ignoring the whole principle of his discussion.

The index is well compiled, and the marginal cross-references if continued throughout the book would have greatly added to its usefulness as a book of reference. The pages on bibliography and current periodical literature are useful, and would have been more useful still to the uninitiated had some guidance been given as to which of the periodicals might perhaps have been suited to him. In fact, why not thus mention the *Observatory*?

The proof-reading has been exceptionally careful; we find one hyphen displaced on p. 342!

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For the extremely ignorant and for the extremely learned in Astronomy the book is not suited, but for that very reason it will probably meet the requirements of many among that larger public who fall between those two extremes. J. A. H.

A READER and occasional correspondent writes to us about a review that appeared in our last number:—

"It seems to me that the reviewer of Dr. Spitta's new book on 'Microscopy,' whilst acknowledging the value of the work, is unnecessarily severe in some of his remarks. Possibly the opening chapter of the book under review is written in a style which is too 'popular' to be compatible with scientific accuracy; the author's experience as a lecturer might easily lead him to err on that side rather than to frighten novices away by the alternative of a strict mathematical investigation of the properties of lenses. At all events, the later chapters make it abundantly clear that it is not want of knowledge which accounts for whatever shortcomings a reader familiar with advanced optics might detect in the opening chapter. Your reviewer's treatment of it might, to modify his own simile, be compared to a man preparing to shoot a sparrow with a 12-inch armour-piercing shell!

"No doubt it is very wicked to use the same word 'focus' indifferently for either a point or a length; but who has *not* been guilty of speaking of a telescope of, say, 6 feet (or even foot!!) focus when he should have stated its equivalent focal length? Life is too short to allow one to be quite exact always. And here, again, the author in a later chapter goes more thoroughly into the determination of the true equivalent focal length of lenses than has ever before been done in any book written in the English language.

"Diamond-fronts were no doubt left out of consideration by Dr. Spitta for very good reasons. Apart from the fact that it seems hitherto to have been found impossible to produce optically perfect surfaces on diamonds, a diamond-front would be of use only if associated with an immersion-fluid of approximately the same refractive index. In the present state of chemical and physical knowledge melted phosphorus would seem to be the only liquid fulfilling this condition, and this Dr. Spitta might well hesitate to countenance in a book on *practical* microscopy.

"One may agree with your reviewer that the term 'aplanatic cone' is not a very happy one; it was, however, introduced by Mr. Nelson when he first taught the method of determining the useful or efficient portion of the full aperture of condensers, and the term is now firmly established. Anyone who has waded through a few pages of another recent work on Microscopy, of which the introduction of a host of new and generally dreadful terms forms the most prominent feature, will be grateful to Dr. Spitta for limiting himself to accepted terms, even though they might occasionally be somewhat inadequate or unsuitable."

NOTES.

COMET NOTES.—With the aid of Prof. E. Weiss's ephemeris, Mrs. Fleming has found images of Comet 1904 I. on seven plates taken at Arequipa in May and June 1903. This brings the interval covered by the observations of this comet up to 753 days (*Ast. Nach.* 4202).

It appears that the stars were wrongly identified last month in describing Mr. Grigg's sketch of Comet *b* 1907 on April 8. He gave no orientation in the sketch first sent, but subsequently supplied this. It now appears that most of the stars in his sketch are too faint to be included in the C.P.D., and it is possible to make different selections from the C.P.D. stars to satisfy his sketch within the limits of error, which are evidently somewhat large. I have calculated the position of the comet 1907 April $7^d\ 19^h\ 58^m$ G.M.T. (the time of Mr. Grigg's sketch) from the following elements, which Dr. Weiss gives in *Ast. Nach.* 4203 (from observations on April 15, May 7 and 14):—

T	1907 Mar. 27·611 Berlin M.T.	
ω	328° 47′ 6″	1907·0.
☊	189 2 59	
i	109 39 39	
log q	9·96596	

The calculated position is

R.A. $4^h\ 35^m\ 44^s$. S. Dec. 44° 18′.

It is possible to fit some of the stars of the sketch with a position $4^h\ 36^m\ 6^s$, S. 44° 18′; but the identification could only be rendered certain with the aid of a photograph of the region.

Dr. Holetschek, in *Ast. Nach* 4205, points out that in the published elements of 1813 I. there has been an erratum as regards the sign of the comet's latitude; when this is corrected the elements become:—

T	1813 Mar. 4·5312 Paris M.T.	
ω	170° 38′ 25″	1813·0.
☊	240 35 54	
i	158 50 11	
log q	9·84460	

He further notes an erratum in the elements of 1813 II. in Galle's 'Kometenbahnen'; the inclination should read not 98° 7′, but 98° 57′.

Mr. H. A. Peck, in *Ast. Journ.* 598, finds that the most probable value of the eccentricity of Comet 1894 II. is 0·9899, giving a period of 959 years; the probable error is about half a century.

He also examines the transit of the Sun by Comet 1819 II., finding that ingress took place 1819 June 25·705 G.M.T., egress $3^h\ 36^m$ later. He concludes that the object seen by Pastorff and Stark was not the comet.

A new comet, *e* 1907, of mag. 9·5, was discovered by Mr. Mellish at Madison, Wisc., U.S.A., on Oct. 13, being visible in an opera-glass. The following elements are by Herr Ebell, from observations on Oct. 15, 16, 18 :—

T	1907 Sept. 13·746 Berlin M.T.	
ω	293° 27'·4	1907·0.
☊	54 54·9	
i	119 23·3	
log q......	9·99118	

Ephemeris for Berlin Midnight.

	R.A.	N. Dec.		R.A.	N. Dec.
	h m s	° ′		h m s	° ′
Nov. 2....	6 46 30	5 28	Nov. 14....	3 50 40	23 58
4....	6 24 17	8 26	16....	3 18 16	25 51
6....	5 58 40	11 42	18....	2 48 30	27 7
8....	5 29 42	15 6	20....	2 22 17	27 54
10....	4 57 54	18 27	22....	2 0 4	28 29
12....	4 24 24	21 28			

The brightness is a maximum on Nov. 9, three times that at discovery ; it then declines rapidly, being equal to the discovery brightness on Nov. 25. It is nearest the Earth on Nov. 10 (distance 0·424), in the ascending node Nov. 12, in opposition to the Sun Nov. 16. A. C. D. C.

Minor Planet Notes.—The following planets have received permanent numbers :—TE 602, TJ 603, TK 604, UU 605, VB 606, VC 607, VD 608, VF 609, VK 610, VL 611, VN 612, VP 613, VQ 614, VR 615, VT 616, VY 617, VZ 618, WC 619, WE 620, WJ 621, WP 622, XJ 623, XM 624, XN 625, XO 626, XS 627, XT 628, XU 629, XW 630, YJ 631, YX 632, ZM 633, ZN 634, ZS 635.

The following identities have been established :—UW with 66, UZ with 408, WO with 260, WQ with 167, XK with 129, XL with 462, XZ with 469, YH with 236, YK with 411, YL with 629, YP with 62, YQ with 450, YW with 631, ZA with 510, ZB with 31, 615 with KR and KV, WK probably with 534, 635 with TU, UT probably with GX, YA with YF. The other planets, up to ZR, do not at present receive permanent numbers, owing to insufficient observations ; XP, however, has been sufficiently observed and will be numbered.

Planet 583 has been named Clotilda.

The following new planets have been discovered at Heidelberg:—By Kopff: AA, Sept. 7; AB, AC, AD, AE, AF, Sept. 11; AJ, AK, AL, AM, Oct. 4. By Scheifele: AH, Sept. 18.

Dr. Metcalf, at Taunton, Mass., found AG, Sept. 14.

AL, AM were on a plate exposed for the search for de Vico's Comet, of which no trace was found, in spite of a good sky and an exposure of 3½ hours.

Ast. Nach. 4206 contains a series of observations of 588 Achilles, made with the Yerkes 40-inch refractor, by Prof. Barnard, extending from 1907 Feb. 12 to June 2. A. C. D. C.

OBITUARY. — MAURICE LŒWY. On Tuesday, October 15, a Meeting of the Committee* of the French Astronomical Observatories was being held at the Ministry of Public Instruction, Paris, where M. Lœwy, Director of the National Observatory, Paris, addressed the Meeting, but before he had ended his speech fell back in his chair and died almost instantaneously from hæmorrhage of the brain. On account of the association of England and France in more than one astronomical undertaking, M. Lœwy could be regarded by many of us as a personal friend, and his death is therefore specially felt. The following eulogy spoken by M. Becquerel at the Meeting of the Academy of Sciences on October 21 contains the facts of his career, and is testimony to the esteem in which he was held by his fellow-workers:—

"Since our last meeting a loss as cruel as unexpected has fallen on the Academy. Our eminent colleague, M. Lœwy, one of our leaders, died suddenly last Tuesday whilst speaking in the Council of the Astronomical Observatories. A beautiful death for a worker who never knew repose.

"To-day, before beginning our work, I wish to recall in a few words the history of our friend whose loss we all deplore.

"Born in 1833 at Vienna, Maurice Lœwy was 27 years old when he was appointed by Le Verrier to the Observatory of Paris. Four years later he was naturalized as a Frenchman, and has done honour to his adopted country. His studies on comets and planets, the idea for a new equatorial instrument that he was able to materialize later, precise determinations of the difference for longitude between Paris and the towns of Algiers, Marseilles, Berlin, and Vienna led him in 1873 to the Academy of Sciences, of which he was made a member in place of Delaunay.

"From that moment his labours multiplied and acquired a practical importance quite remarkable. He planned a simple but precise method of determining the flexure of astronomical telescopes which furnished a solution of this important problem of astronomy of position. The originality of his talent as an observer showed itself in methods for determining rapidly and accurately the absolute coordinates of stars. By an ingenious device he measured directly the variations of relative positions of two stars by causing their images reflected from two solid mirrors to appear in the focal plane of the same instrument, which was applied to the measure of the constant of aberration and to the delicate study of astronomical refraction.

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* The French Colonial and Provincial Observatories—Algiers, Bordeaux, Marseilles, Toulouse, and others—work to some extent in concert with one another and the Paris Observatory, and the Directors occasionally meet in Committee.

"We know also of the admirable photographs of the Moon taken, in conjunction with M. Puiseux, with the beautiful equatorial *coudé* that Lœwy designed; and without insisting on the care necessary for the publication of the 'Connaissance des Temps,' and of other such works, numerous and useful, that he effected, I would tell you that just before his death our colleague passed the proofs of an important memoir upon a method for determining the division errors for a divided circle.

"Lœwy was one of those who, since Bessel, has contributed very greatly to progress in methods of high precision in astronomical observation.

"Endowed with a perseverance which would not allow him to be discouraged by any obstacle, he had the force of will to concentrate his intellectual activity on a single object until he had attained the end that he had proposed.

"Last of all, I call to mind the high character and benevolence of heart of our regretted colleague, whose beautiful life of continuous work leaves behind not only its own results, but an example for others to follow."

[Maurice Lœwy, born April 15, 1833, was educated at the Polytechnic School and University of Vienna. Assistant in the Observatory of Vienna, 1856 to 1860. Joined the Paris Observatory in August 1860: Astronome-adjoint, 1861, August 27; Astronome titulaire, 1866, February 14; Sous-Directeur, 1878, June 27; succeeded M. Tisserand as Director, 1896, December 1.]

The funeral of M. Lœwy was attended by Sir David Gill and Major MacMahon, representing the Royal Society, Royal Astronomical Society, and British Association.

The Transit of Mercury.—In the forenoon of November 14 the planet Mercury will pass in front of the Sun's disc. The last occasion when this phenomenon happened was 1894 November 10, the next will be on 1914 November 6. Twenty-two synodic periods of Mercury are pretty nearly equal to 7 years; 41 still more nearly equal 13 years, and 145 almost exactly equal 46 years. Hence after a November transit a second one is possible in 7 years, probable in 13 years, and practically certain in 46 years*. In May, when the Sun's longitude is the same as that of the ascending node of Mercury's orbit, the planet being at aphelion, the repetition after 7 years is not possible and it often fails in 13. It will probably be difficult to see Mercury before it reaches the

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* Young's 'General Astronomy,' p. 351.

Sun, but such observation has been made, and, as is pointed out by Prof. Moye in his letter on p. 415 of this number, something may then be seen which will be evidence of an atmosphere. This is further explained in an article by M. Bigourdan in the *Comptes Rendus* dated October 14 last, who, writing of Venus at the transit of 1882, says: "On commenca de voir cette planète longtemps avant le premier contact; ce n'était d'abord qu'un trait fort brillant de 15″ de long placé sur le bord de la planète opposé au Soleil; il s'allongea graduellement, en se courbant finalement il forma d'auréole, ce qui est une preuve directe de l'existence de l'atmosphère de Vénus; aussi serait-il intéressant de faire la même constatation pour Mercure."

The appearance of a halo or bright ring about the black disc of Mercury in transit has often been seen, but it has been set down as a subjective ocular phenomenon. At some previous transits small bright spots on the black disc have been seen, but the reality of these is also doubtful.

It may be well to point out that the times of external and internal contact at Ingress and Egress given in the 'Nautical Almanac' and in the "Companion" are the times of the geocentric phenomena. The times for any place on the Earth's surface can be computed by the formula given in the N. A. p. 451. For Greenwich

		d	h	m	s
External Contact, Ingress ..	Nov.	13	22	23	24
Internal ,, ,, ..		13	22	26	1
,, ,, Egress		14	1	48	10
External ,, ,, ...		14	1	50	50

The planet will traverse a chord near the north pole of the Sun (Ingress 62° E. from north point, Egress 345° E.), so that it is not likely to pass near any sun-spots.

SATURN.—As seen from Saturn, the Sun has been south of the plane of the rings since July 26, and according to the ephemeris the Earth crossed from south to north of that plane on October 4. Therefore as the Earth and Sun are now on opposite sides of the ring-system we are looking at the unilluminated side, which should be invisible to us. A casual observation showed that the ring was just faintly visible on October 2, and had disappeared on October 4; but at the Meeting of the British Astronomical Association on October 30, a note by the Rev. T. E. R. Phillips was read, its substance being verified by a subsequent speaker, to the effect that the ring has not completely disappeared, and that there has been distinctly seen, on several occasions at the latter end of October, threads of light on either side of the planet in the position of the ring. This is worth looking for in the present month. Mr. Phillips makes the suggestion that the appearance is caused by the Sun shining through the Cassini division and lighting the edge of the inner ring.

A telegram has come from Prof. Pickering, dated October 21, as follows:—Campbell telegraphs, "Prominent bright knots visible during past week in Saturn's rings—two east, two west, symmetrically placed."

Proposed Longitude Determinations.—The Hydrographer of the Navy, Admiral Mostyn Field, has initiated a telegraphic determination of the longitude of the Island of Ascension. Capt. Monro, R.N., and Lieut. Gibson, R.N., who will make the observations at Ascension, have been at Greenwich, determining their personal equation with respect to the Standard Observer. It is proposed to connect Ascension both with Greenwich and the Cape Observatory, so that incidentally the difference of longitude of these two places will be determined.

The determination of the longitude of Cairo, mentioned by the Astronomer Royal at the June Meeting of the Royal Astronomical Society, has been postponed. It is not unlikely that the longitude of Malta, about which there is at present some uncertainty, will be re-determined shortly.

The Constellation Orion.—A note in *Popular Astronomy* announces that Mr. H. N. Russell has prepared a paper on the probable distance of the constellation of Orion. He thinks that nearly all the bright stars in Orion are roughly at about the same distance from us. These stars are similar in their spectra and their proper motions. Their proper motions being small indicate a small parallax, which means their distances are great. The fact that many of the stars are double, and that probably some are physical binaries, showing motion in rotation, more or less small, makes it possible to find the relation between the parallax and the mass of such systems, the distance between the components and the ratio of the motion. Such data when applied to several systems ought to give an average result of value. If we consider the orbits of two of the binaries already determined, their average is about ten times that of our Sun. This gives a mean parallax of $0''\cdot005$, which makes those stars at a distance of 600 light-years. This interesting and difficult piece of work might well engage the attention of observers competent to do the work as a part of the larger question which concerns the size of the universe.

The Leeds Astronomical Society.—Most people who read Tennyson realize that the poet was a devout stargazer; and in the Journal of the Leeds Society for 1906 there will be found an extensive article by Mr. Whitmell on this subject, in which he has collected some hundreds of quotations derived from 114 poems referring to astronomy, optics, or meteorology. Tennyson, the article says, was at a disadvantage as a naked-eye astronomer, for he was so short-sighted that, unless aided by glasses, he saw the two pointers of the Great Bear as two intersecting circles, like the first proposition in Euclid. This article to a student of poetry is alone well worth the half-crown asked for the publication, but, besides this, it contains an article on the supposed planet inside Mercury's orbit; an account of the opening of the Cecil Duncombe Observatory, which has been equipped for the City of Leeds through the joint agency of Major C. W. E. Duncombe, the Leeds University, and the City Council, and was formally inaugurated by Prof. Turner on 1906, May 4, together with various other useful notes, among which we specially remark a description of the method of stereographic projection by Mr. Whitmell as applied to Mr. Blaikie's spherical slide-rule.

LOWELL EXPEDITION TO THE ANDES. — At the expense of Dr. Percival Lowell, Director of the Flagstaff, Arizona, Observatory, Prof. David Todd, of Amherst College, dismounted his newly-built 18-inch Clark refractor, and transported it to South America in search of a steady atmosphere, with Mars at opposition high in the zenith. The station finally selected was "Alianza," a nitrate oficina, in latitude 21° south, and located in the elevated desert pampa back of Iquique, in the desert of Tarapacá, Northern Chile. Here the night-time sky was almost invariably clear, from June 18 to August 1, during which time the planet was photographed about 7000 times, embracing a complete revolution and every part of its surface.

American plates of Seed & Cramer (orthochromatic and isochromatic instantaneous) were used in a special planetary camera of the Lampland pattern, built by Gaertner of Chicago, and equipped with a Wallace colour-screen.

Prof. Todd tells us that the much discussed canals and oases are abundantly portrayed in this unique series of planetary photographs, as many as 20 canals being sometimes caught at a single exposure, and that the crowning work of the expedition was the photographic record of numerous double canals, among them Euphrates, Thoth, Eumenthes, Dihon, Astabour, Phison, and Nilokeras.

ROYAL METEOROLOGICAL SOCIETY.—The Council of the Royal Meteorological Society at their Meeting on Oct. 16 awarded the Symons Gold Medal for 1908 to Monsieur L. Teisserenc de Bort, of Paris, in recognition of the services which he has rendered to the science of Meteorology.

The Medal was established in memory of the late George James Symons, F.R.S., the founder of the British Rainfall Organization, and is awarded biennially. The presentation will take place at the Annual General Meeting of the Society on January 15th.

A CORRESPONDENCE has been going on in *Nature* (Oct. 3, 17, 24, and 31) between Prof. Karl Pearson and Mr. Hinks on the subject of the application of statistical methods to astronomical facts, illustrated by the paper communicated by Miss Gibson to the 'Monthly Notices' (May 1906) on the relation between parallax, magnitude, and proper motion of stars. Readers are referred to the originals for the precise arguments; but Mr. Hinks appears to urge that the material worked on by astronomers has already been selected by them, and therefore the laws of probability as used in statistical research do not apply.

With reference to M. Lœwy's sudden death, it is curious that Admiral Mouchez and M. Tisserand, his two immediate predecessors in the Directorship, both died with short warning. Charles Delaunay, who was Director for two years whilst Le Verrier had temporarily vacated the office, also met with sudden death, being drowned from a small boat off Cherbourg.

Dr. Doberck, late Director of the Hong Kong Observatory, asks us to say that his address is now permanently Kowloon, Elgin Road, Sutton, Surrey.

Mr. A. S. Eddington, Chief Assistant in the Royal Observatory, Greenwich, has been elected to a Fellowship at Trinity College, Cambridge.

The first Meeting of the Session of the Royal Astronomical Society will be on November 8. The next Meeting of the British Astronomical Association will be on November 27.

From an Oxford Note-Book.

The lamentably sudden death of M. Lœwy continues the series of disasters which have befallen Astronomy in France in recent years. Within five years we have lost Cornu, Faye, Perrotin, Paul and Prosper Henry, Rayet, Callandreau, Bischoffsheim, Bossert, Trépied, and now Lœwy. Of the eleven Associates of the Royal Astronomical Society whose names have disappeared from the list in that period, six were French. The blows have fallen thick and heavy, and recovery will not be easy or rapid. Although M. Lœwy was seventy-five, he was still so vigorous that his death comes as a great shock to us who met him so recently in Paris. He was then full of one of his recent researches, and on his being nominated a week later, by the International Association of Academies, Chairman of the Committee on Lunar Nomenclature, he took up his new duties with responsive zeal. But the serious illness of one of his children tried him severely. He wrote on August 26 (after discussing various matters concerning the above Committee with careful attention) that the grave danger was now past, and that he was leaving Paris for a short period, and added "Je vous assure que j'avais bien besoin de me reposer un peu." It would appear that he had indeed overtaxed his strength. He died suddenly on October 15.

The following extract from a letter from Mr. Evershed, dated from Kodaikanal on September 16, will be interesting to many friends:—

> Bad definition, when it occurs, is invariably due to complex currents of air high up in the clouds. The weather here during June, July, and August is very much disturbed by the S.W. monsoon, but as soon as a break in the

monsoon occurs, and the wind dies down, the early morning definition becomes splendid. The air at this altitude seems to settle down to a homogeneous condition very rapidly after disturbed weather.

We are now expecting the advent of the N.E. monsoon, which is said to be much worse than the S.W. I will reserve my opinion of the Kodai climate until we have completed a year. I bear it a grudge, however, on account of the exasperating clouds always in the region of the comet in the early mornings, and this has effectually prevented my obtaining any really good photographs of the spectrum of Comet Daniel. Those I have secured show an interesting peculiarity in the emission spectrum of the tail. The gas cyanogen gives by far the strongest emission lines, but is confined to the region of the head, whilst the fainter hydrocarbon lines extend quite a long way into the tail. I expect that much better results have been obtained elsewhere, and that this feature has been noted by others.

We are both, my wife and myself, in good health and extremely busy. My wife has six acres of wild garden to bring into shape, which she is doing with the aid of a small army of coolies. She is also making a card catalogue of the library.

A CURIOUS example of either personal or instrumental error is analyzed by Hirayama in *Ast. Nach.* No. 4207. In using a zenith-telescope in reversed positions, the reversal may be either East to West or West to East. Comparing the two cases, systematic differences are found which have been denoted by (O – W) – (W – O) by German astronomers, and Battermann has ascribed them to differences of magnitude between the pair of stars selected. Hirayama finds no such connection, but does find that the differences follow the Zenith Distance of the pair observed, changing sign at the zenith. There are features in this discordance common also to the R – D discordance. This latter has been shown* to change according as the R observation or the D observation is made first, *i. e.* to give an (R – D) – (D – R) discordance. Hirayama is looking for an origin in variation of personality with the declination: but is not the origin in both cases more likely to be found in some gradual change of position in the instrument, due to slow yielding to stress?

THE Moon's east limb has been shown by Mr. Walter Heath, from a discussion of occultation-residuals, to be very sensibly different from a circle. From position-angle 67° to 95° he finds a mean radius of $15' \, 33''\cdot37$, while from 105° to 116° he gets $15' \, 30''\cdot50$, the difference being $2''\cdot87$. This is for libration $-4^\circ\cdot5$ (*Ast. Nach.* No. 4206). These results are not only significant comments on the value of measures made on the limb (for determining positions either of the Moon in space or of objects on its surface), but acquire a new interest in view of the line of work started by Jeans and Love on the Earth's figure. It seems possible that, by one method and another, we may learn something of the Moon's figure, and so further test the hypothesis of "gravitational instability."

IT has long been a vexed question whether increased exposure, in the case of stellar photography, is the exact equivalent of increased brightness; whether, for instance, when the exposure is increased 100 times (the logarithm of which is 2·0 or 5 × 0·4) we get stars 5 magnitudes fainter than before. It has been claimed that we do, and it has more often been asserted that we get something short—even something considerably short. Evidence seems to be accumulating that for star work the shortage is about 1 magnitude in 5; that with 100 times the exposure we get only 4 magnitudes fainter instead of 5. This result is stated in Mon. Not. lxv. p. 775; it has been obtained independently by Mr. Ejnar Hertzsprung, of Kopenhagen (*Ast. Nach.* No. 4204), and it has been deduced from Dr. Schwarzschild's far-reaching investigations, which are of a different character. The two results first quoted are obtained from the measures of diameters of star-discs in approximate focus; Dr. Schwarzschild and Prof. Pickering have worked with star-discs *out of focus*, which are accordingly of nearly constant diameter, and differ chiefly in density. Dr. Schwarzschild has, moreover, made laboratory experiments to test the behaviour of plates. I am indebted to Dr. L. de Ball for the following summary of results obtained in different ways. They are presented as the effect in magnitudes of 100 times the exposure, so that what should be 5·0 comes out 4·4 in the laboratory and 4·0 for stars:—

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* See Mon. Not. R. A. S. liv. p. 488, and Mem. R. A. S. vol. li.

In the Laboratory.	
Abney (bromide of silver paper)	3·9
Abney (Wratten & Wainwright plate)	4·1
Michalke (Herzogplatte)	4·6
Schwarzschild (Schleussner plates)	4·4
Schwarzschild (Leunière plates)	4·8
Schwarzschild (Schattera plates)	4·3
Schwarzschild (Anilinfabrik plates)	4·3
Schwarzschild (Smith plates)	4·8
Mean	4·4

On Stars.	
Charlier (diameters)	3·7
Scheiner "	4·3
Trépied "	3·8
Christie "	4·8
Scheiner (faint stars)	(3·0)
Dunér "	3·8
Schwarzschild (images out of focus)	3·7
Schwarzschild	3·8
Mean	4·0

A very readable account of Dr. Schwarzschild's work is given by Dr. L. de Ball in the *Bulletin Astronomique* for October and November 1905, though the above point is not there dealt with.

FROM a correspondent:—"I was looking at a set of gramme weights in the shop of a London optician the other day. The optician told me that a customer had seen them a short time before, and had liked the look of them; 'but,' said he, 'as I travel about a good deal, I should like a set made in aluminium.'"

FROM 'The Isle of Unrest,' by J. S. Merriman. The Colonel had just had dinner, about seven o'clock in the evening. The description then goes on:—

> Here the Colonel seated himself . . . . and sat watching a young Moon rise from the sea.

FROM Lever's 'Charles O'Malley,' date referred to being 1815, June 15, when the hero was leaving the Duchess of Richmond's ball at Brussels :—

> The thin pale crescent of a new Moon .... tipped with its faint sickly light the tall minarets of the Hôtel de Ville.... Although midnight, the streets were crowded.... The deep bell of the Hôtel de Ville tolled one.... The faint Moon only shewed at intervals.

A CORRESPONDENT has been meditating on the following sentence from an excellent little book on the Moon by Dr. Franz, in the chapter entitled "Beobachtungen eines Monbewohners" :—

> Insbesondere lassen sich die R.A. der Sterne aus Beobachtungen ihrer Durchgänge durch das Fadennetz eines Fernrohrs ungleich genauer finden, als auf irdischen Sternwarten, weil die scheinbare Umlaufbewegung des Sternhimmels 27½ mal so langsam sich vollzieht.

He points out that there are a number of other circumstances entering into the problem, such as, for instance, the small value of gravity, which might allow changes in length of the clock-pendulum to assert themselves, and so increase accidental error in time determinations. But Dr. Franz's main contention seems to both of us to be an oversight, for we cannot see that the R.A.s of stars depend for their accuracy on the rate of apparent motion. There is a false appearance of such dependence when the variable unit is employed instead of the equatorial interval; but it goes the other way, the slow-moving stars seeming *less* accurate. Again, let us suppose a "registering-micrometer" used: would not lunarians be much in the same position as ourselves? But perhaps some one else may hit upon the idea in the mind of the writer.

A VERY amusing article on "The Man in the Moon," by Mr. Arthur Mee, appeared in the "Humour Number" of the *London Magazine*, pp. 575–579. By a number of good illustrations it is shown how various people have seen in the Moon, Cain, Cain and Abel, "the Lunar Kiss," a woman reading, an old woman feeding spaniel, &c., &c. But these are as nothing compared with the discoveries of Mr. G. Topping, of Plymouth. According to the *Western Morning News* (of September 21?) he commenced these discoveries some 14 years ago, on rocks and skeletons. But on a fortunate day certain astronomical photographs came into his possession, and markings attracted his attention which he instantly saw were similar to those he had found in the animal and mineral worlds. His account of what he sees on the Moon is best given in the words ascribed to him :—

> "If one wishes to see these figures," he said, "the one nearest the east is in the shape of a gigantic dark face in profile, having rather prominent lips, and the eye is also present. But the peculiarity of this figure is that just beyond the brow, and towards the back of the head, another figure makes its appearance, and has the appearance of a lamb. This lamb has a large white cross in the

fore part of its face, and the eye is closer, while a terrible gash appears to have been inflicted on the head. These two figures are intersected by another remarkable figure, which has the appearance of a large white countenance in profile, also looking eastward. The back of the head of this particular figure has the formation of an ox. Just above the eyes, and passing through the lower part of the lamb's face, is another human countenance, wearing a kind of crown over the headdress. The cross, which is in the fore part of the lamb's face, actually becomes the nose of this particular figure. Within the lips of the first figure appears another remarkable countenance in three-quarter face, possessing two piercing eyes which appear to be looking down attentively towards the Earth. Within the larger figures there are other smaller symbolical figures, one of which represents a human countenance, as if in terrible agony, with hair streaming in the wind, the mouth wide open, and the eyes staring as if in terror."

"What is your interpretation of these various figures?" asked our representative.

"I am satisfied, in my own mind," was the reply, "as to their interpretation, but I would like to hear the opinions of others before giving my views."

EXTRACTS from a work on "the weather-forces of the planetary atmospheres":—

The irregular meteorological changes are caused by irritation of those places on the Sun which are opposite to certain planetary conjunctions, and the reflection of this irritation to the Earth. This period usually lasts 5 days, viz. 1–2 days of wind and rain, 1–2 days brighter weather, again two days rain, but with much less wind. . . .

If planetary radiation were accepted the programme of the work to follow would be settled. First arises the question:—Which of the planets have an influence on the weather? We have the answer in Rule IV.: "Those with a sufficiently dense atmosphere." . . . .

If subventions are begged for from the governments, the present meteorologists will discountenance them, though an exhaustive, thorough examination of the effect of the influence Mercury-Uranus, with Merc-Saturn and Venus-Jupiter, would not cost more than £40 (200 dollars). Except for observations and purposes of sport: rain-rockets, balloons, hail-cannons and registers, all subventions are declined. . . . These pages contain the results of eight en years' labour, and at the end of it I have to confess I am at present alone in the world. But that is no proof against my principle. . . . Is it not a sublime idea that the bread and meat which we eat, the milk and wine which we drink, are the heavenly gifts of our brothers the planets and of our mother the Sun?

SS CYGNI.—With reference to the note published last month, Prof. Pickering has sent the welcome information that a large amount of material collected at Harvard is nearly ready for publication. There are 1900 visual observations and 1100 photographic, the latter covering several years previous to the discovery of the variable, from the already famous resources of the Harvard Observatory. Had this been known, the note of last month would scarcely have been written; but since it was written, the best course seems to be to repeat the offer to collect and discuss any material which has not been communicated to Harvard. The discussion will be all the easier when once we have before us the magnificent Harvard contribution—if, indeed, there is left anything to discuss.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

VOL. XXX. DECEMBER, 1907. No. 390.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1907 November 8.

Mr. H. F. NEWALL, M.A., F.R.S., *President*, in the Chair.

Secretaries: T. LEWIS and S. A. SAUNDER, M.A.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Saunder announced that 160 presents had been received since the last Meeting, including, amongst others:—W. W. Bryant, 'History of Astronomy'; S. W. Burnham, 'General Catalogue of Double Stars' (2 vols.); and Sir G. H. Darwin, 'Scientific Papers,' vol. i., presented by the authors. Galileo, 'Opere,' edizio nazionale, vol. 3, part 2, and vol. 19, presented by the Italian Government; Lady Huggins, 'Memoirs of Agnes and Ellen Clerke,' presented by the author. Oxford Astrographic Catalogue, vol. 3; Lœwy, Observatoire de Paris, 'Atlas photographique de la Lune' (Lœwy and Puiseux), fasc. 9; and Perth Observatory, Western Australia, 'Catalogue of 420 Standard Stars': presented by the Observatories. Sir N. Lockyer, 'Report of the Eclipse Expedition to Majorca,' presented by the Solar Physics Committee. Astrographic Chart: 134 Charts (Algiers, Bordeaux, Paris, and San Fernando), presented by the French Government; 23 Charts presented by the Tacubaya Observatory, Mexico; and 76 Charts presented by the Royal Observatory, Greenwich. Three transparencies of the Moon from negatives by M. Puiseux, presented by Mr. Knobel; nine transparencies of the Northern Milky Way, presented by Prof. Max Wolf; Medal to commemorate the Benjamin Franklin Bicentenary (bronze), presented by the American Philosophical Society.

The President. I think it would be unnatural not to make reference to the deaths of three astronomers which have happened since our last Meeting, all of whom were Associates of the Society—

M. Trépied of Algiers, Prof. Vogel of Potsdam, and M. Lœwy (Director of the Paris Observatory); this seems a fitting opportunity to make recognition of the great loss Astronomy has suffered. We were represented at M. Lœwy's funeral by the presence of Sir David Gill and Major McMahon, who attended the ceremony at Paris. It is unusual, fortunately, that so many heavy losses fall upon the Society within such a short time.

One of the papers before us is on the subject of Pogson's observations of Variable Stars, the material of which has been worked up by Prof. Turner, who, I hope, will tell us something about the paper.

Prof. Turner. I am glad to have an opportunity of saying one word in order to correct in rather an important detail the statement which has been made by the President. The work was started by me, but has been carried out almost entirely by Mr. C. L. Brook, who is a well-known variable-star observer; and I am glad to have the opportunity of adding that he has generously offered to contribute one-third of the cost of printing. Those who know Mr. Brook already will not be surprised to hear of this new instance of his generosity. It is also a pleasure to add that, perhaps partly stimulated by this individual generosity, the Indian Government has promised to contribute another third to the cost of printing, so that the Society will only have to contribute the remaining third. About one-third of the observations of variables were made after Mr. Pogson had left England for India (*i. e.* they were made in Madras), so that the contribution of the Indian Government is not only generous but also just; it is about the right proportion. The observations are classical ones; they are really pioneer observations. They began about 1857. At that time the method of observing variable stars was only half settled. Mr. Pogson's brother-in-law, Mr. Baxendell, who was observing about the same time, just then changed his method of expression of relative magnitudes from the old system started by Sir William Herschel to the modern method. We have almost forgotten how unfamiliar the estimations in steps were until Argelander started them, and we have almost forgotten how new Pogson's scale was. Those who care to consult Sir John Herschel's 'Outlines of Astronomy' in any edition, even up to 1880, will find he advocates a totally different system of star-magnitudes which we have practically forgotten, a system whereby a star should be supposed to be of the 1st magnitude at unit distance, at 2nd magnitude at two units, 9th magnitude at nine units, and so on, so that the difference between a 9th and 10th magnitude star was very small. Sir John Herschel, however, gave good reasons for adopting that system, and it serves as a reminder of the revolution in estimating star-magnitude which Pogson introduced. Of the records themselves there is not much to be said, and I hope that what there is will be said by Mr. Brook at a future Meeting when he is able to be present, for it is he who has really done the work. But I am glad to have this opportunity of mentioning his generosity and that of the Indian Government, which really is a great compliment to the Society, as well as a help to Science.

After thanking Prof. Turner, the President called on Mr. Crommelin to read the paper standing in his name.

Mr. Crommelin. As Fellows may remember, there were some numerical differences between Mr. Cowell and Mr. Neville in discussing these old eclipses, so I thought it was desirable that the numerical accuracy of Mr. Cowell himself should be put beyond question. Therefore I undertook to test this by an independent computation. I am glad to say the results in all cases agree with Mr. Cowell, with no difference exceeding two seconds of arc, which is a quantity of absolutely no importance at that remote period. Afterwards some smaller terms in the Moon's latitude were included, as it was possible the accumulation of smaller terms might make sensible errors. It was found that, though they modified the residuals to a small extent, they did not at all alter the main drift of Mr. Cowell's argument; they reduced the apparent acceleration of the Sun by about half a second of arc, from 4″ to 3½″, which is satisfactory as being in the right direction. With regard to the Babylonian eclipse of 1062: since the discussion by Mr. Cowell on that subject appeared, we have had Mr. King's book, which gives a full translation of the inscription in which the eclipse is mentioned, and, reading that translation, there are two points in it which seem of importance with regard to this eclipse. One is that the whole document is an account of a series of omens occurring in the city of Babylon. It tells of dogs entering the temples and wild beasts entering the city, so that this makes it clear that the omen, whatever it was, was seen in Babylon itself, viz., it was not an eclipse seen at a distance of many miles from Babylon, as was thought possible at first. Then the phenomenon occurring on the 26th of Sivan (or the 26th of any month in a lunar system of months) makes it certain it was within a day or two of the New Moon. (As the month began from the first observation of the Moon, the 26th would be a little early unless the reckoning had been carried on a month or two, the actual New Moon not having been seen on the previous occasion owing to bad weather.) And that increases the probability that the phenomenon was really an eclipse, not a darkening by atmospheric causes. With regard to the suggested explanation of the apparent acceleration of the Sun, one can only repeat what Mr. Cowell said about tidal retardation. The point is such a simple one that it seems worth while to re-state it in other words, in the hope that Mr. Newcomb and Mr. Neville may indicate more clearly their reasons for dissenting from it.

Prof. Turner. Prof. Newcomb's reason for dissenting from that argument has been clearly stated by him, and it is that, so far, Mr. Cowell's hypothesis is not supported by modern observations of the Sun. To that Mr. Cowell has made reply that some long inequality might compensate the action. We all, I am sure, feel the greatest interest in the campaign which Mr. Cowell has undertaken in the face of many difficulties, and it is none the less interesting because as he goes forward he seems to have to carry more and more weight. If he manages to carry his point ultimately it will be all the greater triumph to him to have encountered opposition from men of such eminence as Prof. Newcomb and Mr. Neville.

Mr. Hinks presented his paper, which consisted of tables for computing standard coordinates of stars on astrographic plates, explaining it with the help of slides on the screen.

Prof. Turner said :—Mr. Hinks has got the formulæ down to a very simple point indeed, and I think the tables are bound to be very useful. There is one point in the calculation where some people might not have the requisite tables, and that is in the $\log \cos \delta$. The number of ways in which this formula may be expressed is probably infinite, and the differences between one and another are due to some quite casual circumstance. Mr. Hinks has assumed the existence of tables giving $\log \cos \delta$ for every second of arc at least. Now some people might not have logarithm tables to a second of arc in their Observatory, and others might. If they do not have them, then some other rearrangement might be preferable. At Oxford we assumed that it was desirable not to use them, the logarithm tables to a second of arc being unwieldy. But assuming that they are used, I think Mr. Hinks has got the formulæ as simple as they can possibly be put.

Mr. Lewis read an account sent by *Mr. Innes* of the disappearance of Saturn's ring as he had seen it with a 9-inch reflector lately set up at the Transvaal Observatory, Johannesburg. The following is a summary of the observations made by Mr. Innes and his son :—

1907 Oct. 3,	$4^h\ 45^m$ G.M.T. ..	Ring seen easily.
	7 30 " ..	Ring difficult but distinct.
	8 0 " ..	Ring still seen.
	9 45 " ..	Ring still glimpsed.
	10 30 " ..	Ring invisible.

Mr. Innes added :—" Observations during the period that the dark side of the ring is exposed to the Earth answer the question as to whether the rings are composed of solid particles or of vaporous matter. If the latter, the edges at least of the rings would be seen."

Mr. Thwaites. On October 23rd I saw the ring very faintly with a 9-inch refractor. It appeared like a solar prominence, diffused at a short distance from the limb of the planet, and very very faint. This was not due to any physiological effect, for

I cut out the planet by an occulting bar, and still the ring could be seen on either side, preceding and following.

Dr. Rambaut. Of course it is well known that the Earth passed to the other side of the ring-plane on the 3rd of October. I mean by the other side the side opposite to that above which the Sun is elevated, and since then the Earth and the Sun have been and still are on different sides of the ring, so that for that reason the rings ought to be invisible. I happened to be showing Saturn to visitors on October 4, and my attention was naturally drawn to this point, and we quite failed to see the ring with a 10-inch equatorial. On the following night I looked with the 18-inch equatorial, specially with the view of seeing whether the ring was visible or not, and failed to find a trace. Since then we have had no opportunity of repeating the observation until a few days ago, when it ought theoretically to have been still invisible, but the ring was distinctly seen, though faintly, on both sides of the planet on November 6.

Mr. H. P. Hollis. Mr. T. E. R. Phillips is not here to tell us his experience, but I happen to know that that gentleman, who is a skilful planetary observer, has been seeing the ring all through October, in fact to him the ring has not disappeared. Mr. Phillips suggests that the Sun is shining through Cassini's division and illuminating the edge of the inner ring, and it is this lit-up edge that is being seen by several persons. I looked at Saturn on October 4, and did not see any trace of the ring. Since hearing of Mr. Phillips's observations I have asked the transit-circle observers at Greenwich, and no one seems to have noticed any trace of the ring, but of course they did not look for it specially.

Dr. Rambaut. I should like to add that it was my two assistants, Mr. Wickham and Mr. Robertson, who saw the ring on November 6.

Mr. Newbegin. I can confirm what Mr. Thwaites has said. If I try to give an idea of what we saw of the ghostly ring, I should say that we saw something faint and detached on either side of the planet, while crossing the planet was a distinct dark line. What we saw was very difficult to hold; we saw these two threads, then lost them, and saw them again, sometimes one, sometimes the other, and at times we could see both.

Mr. Maw. I got a glimpse of the ring on October 11, but it was a very brief glimpse. The whole observation did not last more than a couple of minutes, and I could scarcely believe it. I should not have liked to mention it if I had not been encouraged to do so by what has already been said. I seemed to see just the inner part—about half or two-thirds of the usual length of the ansæ.

Mr. Newbegin. Mr. Phillips, writing to me, said that he saw the ring on the 19th very faintly, and again on the 23rd more distinctly. On that evening it was seen also by friends, and again on the 25th.

The President. It is interesting to note that no one has mentioned Prof. Campbell's observations, which is announced in a recent number of the *Astronomische Nachrichten.* He has quite recently seen knots, as I understand it, on the ring of Saturn. I gather from those who have spoken that what they have seen was rather a suggestion of a line.

Mr. Thwaites. Like a faint detached prominence.

Mr. Michie-Smith proceeded to give an account of a paper by *Mr. Evershed.* As is well known, the peculiar sun-spot spectrum has hitherto only been obtained in the visible region, and it has been stated fairly definitely, by several observers, that the same characteristics are not to be found in the ultra-violet regions. But fortunately at the Observatory several photographs have recently been got which show exactly the same characteristics in the ultra-violet spectrum as in the visual spectrum, and this paper deals in detail with that spectrum. The method employed there for getting sun-spot spectra was to very carefully shut out everything except the central umbra of the spot for a certain length of time, and then, after a sufficiently long exposure had been given to that, to lengthen the slit so as to expose on both sides of the spot, so as to get an ordinary spectrum. The exposure required for the umbra may be as much as twenty times that required for the photosphere.

The plates that will be shown were taken partly with the special apparatus set up for the purpose by Mr. Evershed, using a plane grating, and partly with the parabolic grating which I had specially ruled for this purpose a few years ago. The parabolic grating when used suitably gives very little astigmatism, and it is therefore possible to get excellent spot-spectra.

Mr. Michie-Smith, after showing the plates, said:—I think enough has been seen to show that the problem of photographing sun-spot spectra has been completely solved—that is, when the spot is large enough to treat it in the way that this spot has been treated.

Mr. Michie-Smith then read a short paper by *Mr. Evershed* on the Spectrum of Comet 1907 *d* (Daniel), and showed a photograph of the comet taken by Mr. Nagaraja on August 28, and one of the spectrum taken by Mr. and Mrs. Evershed on the same date. The latter was taken with a prismatic camera, and showed a number of monochromatic images of the comet. The wave-lengths corresponding to the various images were determined by comparison with a star-spectrum taken on one of the comet plates. The paper gave details of the measurements of the plates, but the only lines that could certainly be identified were those due to cyanogen.

The President. It is interesting to notice that the cyanogen band, although far the brightest, is mainly restricted to the nucleus, whilst the other bands, presumably due to hydrocarbons, come down, showing their presence in the tail as well as in the nucleus.

Prof. Fowler. At the June meeting of the Society I had the pleasure of giving an account of my identification of a great number of umbra lines in the spectra of sun-spots with the components of the flutings of magnesium hydride. Since then progress has been made in two directions: first, by obtaining improved photographs in the laboratory, and, secondly, by taking advantage of the beautiful photographs lately taken at Kodaikanal, copies of which have been kindly placed at my disposal by Mr. Evershed. It may be of interest to some present to see how completely my identification is established by the new comparisons which I have prepared. [Screen.] To make the coincidences clearer, my prismatic spectrum has been corrected to the normal scale corresponding to that of the solar spectra, which were taken with a grating. It will be seen that the details of the magnesium-hydride band in the green are very faithfully reproduced in the spot, the head at 5211 being especially obvious. In this particular band I have measured about 400 lines extending from 5211 to near F, and many hundreds more are found in other parts of the spectrum.

There is another point of interest which is brought out by this comparison. The spot-darkness, as observed by Young and others, is not continuous, but is broken up here and there by bright spaces which are of the same order of intensity as the adjacent photospheric spectrum outside the spot. The magnesium-hydride photograph shown on the screen is a negative, and it is readily seen that the bright spaces in the spot correspond exactly in position with clear gaps in the flutings, thus showing that we have to deal with interspaces in the spot and not with true bright lines and bands, as has sometimes been supposed.

In regard to the interesting paper read by Mr. Michie-Smith, I should like to congratulate him on the successful work that is being done in India. The ultra-violet spot mystery appears to be now on the way to being cleared up. It certainly seemed impossible to believe that the spots could be so self-contradictory that in the visible spectrum there should be marked indications of reduction of temperature, while in the ultra-violet there should be no apparent change at all. Prof. Hale has already suggested that the obliteration of the spot-detail might be due to the diffusion of general solar light over the spot; and this seems to be getting more probable as the work proceeds, because with improved conditions greater detail appears to be brought out.

Father Cortie. Does Mr. Evershed give a list of the lines affected in the sun-spots?

Mr. Michie-Smith. Yes; a list is given in the Table.

Father Cortie. It will interest me to compare the wave-lengths with those of some lines which I think are widened in a photograph of the spectrum of the spot of October 29, 1905, which was taken in the ultra-violet. The plate covers the region λ 3700–4415, and a lantern-plate of a portion of it is now on the screen.

The image of the Sun on the slit was only three-quarters of an inch in diameter, so that the image of the spot was very small. Hence the same exposure was given to the spot and the photosphere. When I showed this plate to our President, he expressed a doubt as to whether the widening shown was wholly due to the spot. Still, some lines seem to be due to the spot, as, for instance, this one (λ 4065·24 Mn. Ti.). According to Prof. Hale the sun-spot spectrum is masked in the ultra-violet by the photospheric light scattered by the atmosphere during the exposure, which is relatively much stronger photographically in the violet than in the yellow and red. But if in a photograph which was taken without isolating the spot, and giving it a larger exposure than the photosphere, spot-lines are evident, it would seem that this opinion would have to be modified.

The President asked Mr. Eddington to comment on a paper received from the Royal Observatory.

Mr. Eddington. Before I say anything about the proper motions of astrographic reference-stars, perhaps I may refer back to the paper on the spectrum of Comet 1907 *d* as an excuse for showing on the screen three photographs of that comet that have been taken at Greenwich. These photographs were taken by Mr. Melotte with the 30-inch reflector—this one was taken with an exposure of 12 minutes; that one with 13; and that one with 7 minutes.

As to the paper on proper motions, I was not expecting to have to say anything about that; we have already had, rather more than a year ago, a paper by Mr. Thackeray explaining their nature. The Greenwich 9-year Catalogue for Epoch 1900 includes reference-stars for the Astrographic Chart from the pole to north declination 64°. Stars within 9 degrees of the pole were observed by Carrington about the year 1855; and these proper motions are derived from a comparison of the two catalogues. There are 1186 stars; so that, considering they are all within 9 degrees of the pole, it is a very complete list of proper motions for that region. The stars are all of magnitudes down to 9·5. Of course the great majority of them are between the magnitudes 8·5 and 9·5; and we have had hitherto very little data as to the motions of the stars between those limits. I think most of the photographic proper motions that have been determined concern stars fainter than that magnitude; and most of those determined in the ordinary way are brighter. One criticism might be made —a proper motion is given for every star, although, in cases where the proper motion is very small, the quantity given may be practically accidental error. That seems necessary in a paper intended rather as a basis for statistical discussion; it is generally best not to attempt to distinguish between stars that may be said to have proper motions and stars that have not, because there can be no hard-and-fast line.

Dr. Lockyer read a paper on the Permanency of some Photo-

Visual Lenses, which was an account of experiments on the growth of crystalline matter between the lenses of some objectives belonging to the Solar Physics Observatory. Each of the lenses in question consists of a combination of three elements, the front and back lenses being double convex, and the middle double concave. The front and middle lenses are nearly in contact, but a small air-space separates the middle from the back lens. The front lens is made of Schott's baryta light flint glass, the middle lens is of Schott's boro-silicate flint glass, while the third is made of a light silicate crown.

The experiments, the results of which were exhibited in some beautiful photographs, showed that a crystallization formed on the inner surfaces of the two outer lenses, but not on the middle lens. This was thought to be due to moisture and excess of alkaline in the glass.

Mr. W. H. Maw here took the Chair whilst Mr. Newall read a paper written by himself.

Mr. Newall. It seemed to me it might be of interest to give a short account of the observations which I have been making during the past summer, chiefly relating to the cyanogen bands in the solar spectrum. The presence of cyanogen seems to be indicated by the distinct appearance of the absorption bands at wave-length 3883 in the ultra-violet part of the solar spectrum. But, so far as I am aware *, we have no definite observations to show whether this cyanogen is in the solar atmosphere or between the Sun and the Earth, not connected with what we regard as the true solar atmosphere. During the past summer I have put the question to the test by attempting to determine the rotation-period of the Sun by the spectroscopic method, and thus seeing whether the period deduced from the cyanogen bands coincides with that deduced from neighbouring iron lines. This method is simply a modification of Cornu's well-known method of distinguishing between lines of solar origin and telluric origin. It was in that way the oxygen lines at the red end of the spectrum were definitely determined to be of telluric origin, while the neighbouring iron lines were shown to be of solar origin. In the result I find the photographs show displacements of the cyanogen lines similar to those exhibited by the neighbouring lines of iron, chromium, and other vapours. But there are also evidences in the photographs that there is a trace of cyanogen bands unaffected by solar rotation. Thus we have cyanogen in the solar atmosphere rotating with the Sun, and also signs of it in the space between the Sun and the Earth. Whether the non-rotating vapour is in the Earth's atmosphere or outside of what we regard as the limits of our atmosphere is a question to be decided independently. In considering whether it was worth while to attack this investigation, I had other considerations in my mind which have been there for several years, relating chiefly to the question of spectrum of comets. It is usually assumed that a comet brings with it the cyanogen and hydro-carbon vapours which give rise to the greater part of its luminosity; that is, the comet brings these gases from space into the neighbourhood of the Sun. I have often wondered whether it would not be more rational to consider that the comet returns with its solid parts, and evokes out of those vapours already in the neighbourhood of the Sun the spectra of the gases that always appear in the comet. Are we to consider, in fact, that all comets bring the same kinds of gases with them, wherever they come from and whatever the main material of their solid parts may be; or is the constancy of the spectrum of the comet due to the constancy of the vapours found about the Sun? I recognize that it is dangerous work touching the theory of the phenomena of comets; but there are such a number of observations all converging in the same direction, that they seemed to justify me in devoting some time to making special observations; for one or two facts are better than a thousand theories. So putting it to the test, I have been interested in finding a definite answer, at any rate in one respect, viz., that we have proof that those bands of cyanogen that we see in the solar spectrum are partly due to the presence of that vapour in the solar atmosphere rotating with the Sun. But I hardly know yet how to co-ordinate the various peculiar details that I have noticed in the spectra, details which it is rather difficult to summarize, especially when one has been spending a few weeks in rather severe critical examination of the evidence. But I have come to the conclusion that the peculiar effects photographed are to be accepted as real, as indicating real differences in the spectra, and not as being due to instrumental defects. The work has been done with a new spectroscope, which we owe to the munificence of the late Mr. Frank McClean; it has been put up at Cambridge as part of the installation for solar work at the observatory.

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* [*Note.*—It had escaped my memory that bright cyanogen bands have been recorded in the flash-spectrum by Sir N. Lockyer's party in the Indian eclipse of 1898, and also by Prof. Dyson in Sumatra in 1901. I am indebted to Prof. Dyson for reminding me of these observations, which prove that cyanogen is present in the chromosphere. This is a point that I ought not to have overlooked, but it would not have deterred me from making the observations dealt with in my note.—H. F. N., Nov. 20.]

I think I may best sum up the results that I have obtained by showing you quickly a succession of slides. [Lantern.]

[Attention was called to the marked difference in definition in the spectra of the east and west limbs when photographed in quick succession. Mr. Newall stated that it had been observed as persisting, in the neighbourhood of the cyanogen bands, in greater or less degree for nine days, between September 6 and 15. The cyanogen band, in particular, showed differences in the two limbs; there were signs of distinct duplication of the head of the band at the east limb, one head partaking of the rotation of the Sun, the other showing considerable motion towards the Earth; at the west limb the head of the band seemed only to be abnormally intensified. There were many other minor differences between the spectra of the limbs near the cyanogen band. There were difficulties about attributing the changes to a local disturbance on the Sun, but it seemed clear that the changes must be due to vapours nearer the Sun than the Earth. He made reference to the fact pointed out by Kritzinger, that the Earth had passed close to the orbit of Daniel's Comet 1907 *d* on September 12, at a part which the comet had passed on July 27, on its way to perihelion on September 4. He concluded by saying:—I hesitate to do more than set these facts before you, and leave everyone to draw his own inferences from them.]

Prof. Fowler. Is there any evidence that cyanogen in a cold state is capable of producing that kind of absorption-band in the spectrum?

Mr. Newall. Any vibrating system is capable of absorbing.

Prof. Fowler. Will cold cyanogen produce the same absorption as cyanogen in a hot state?

Mr. Newall. It depends on the range of that physical state of the particles producing absorption. Sodium vapour must be hot in the Sun, and yet is capable of absorbing the light of the D lines there. We know that at much lower temperatures it absorbs the D light also. But one can get it at temperatures at which it does not absorb. It is simply a question of the range of the life of that power of absorption.

Prof. Fowler. I had the privilege of seeing the paper in MS., and I may say there is a great deal more in it than our President gave us. I hope, therefore, he will refer to the subject another time.

Mr. Maw. We hope to have the opportunity of discussing this exceedingly interesting and suggestive paper more fully another time than is possible to-day. I have always thought it a great pity that the lateness of the hour should prevent an interesting paper being thoroughly discussed, and I see no reason why the discussion should not be resumed at a future Meeting; anyhow, I hope our President will allow that to be done in this case. Presidents do not always communicate papers, so I think we may congratulate ourselves on the fact that Mr. Newall, in becoming President, has not deprived us in this respect. We hope he will maintain this good habit, and give us many more papers as interesting as this.

Mr. Michie-Smith showed a number of spectroheliograms taken by Mr. Evershed at Kodaikanal. Amongst these were a pair of plates taken on May 11 and 13, in the former of which a prominence was shown on the limb, while in the latter the same prominence was clearly seen as a dark marking on the disc. Another pair of slides showed on an enlarged scale the characteristic structure of flocculi in the earlier and more condensed stage and in the later disintegrating stage. A spectrogram of the limb on April 9 showed a large prominence of the very unstable class of prominence described by Young as seen by him on September 7, 1871. Another slide showed an eruptive prominence on May 25, in which a bright spot was seen leading a train of glowing filaments. This, Mr. Evershed remarks, is often characteristic of prominences which show motion in the line of sight. A spectrogram of August 29 showed in a prominence a black, hook-like marking, which Mr. Evershed considers to be an absorption effect. Several of the prominence plates gave good examples of what he had called attention to at a former Meeting, namely, the way in which prominences at a considerable distance from each other were frequently joined together by delicate horizontal filaments.

The following papers were announced and partly read :—

Max Wolf. "The Nebula ♅ iv. 74 Cygni."

W. J. S. Lockyer. "Note on the Permanency of some Photo-Visual Lenses."

Walter Heath. "Occultation of the Hyades."

R. T. A. Innes. "Disappearance of Saturn's Ring-System 1907 October."

J. Evershed. "The Ultra-Violet Region in Sun-spot Spectra."

J. Evershed. "The Spectrum of Comet *d* 1907 (Daniel)."

A. R. Hinks. "Tables for computing Standard Co-ordinates on Photographic Plates."

A. C. D. Crommelin. "Note on the Ancient Solar Eclipses discussed by Mr. Cowell."

Royal Observatory, Greenwich. "Observations of the Satellite of Neptune from Photographs taken between 1906 December 27 and 1907 April 24."

Royal Observatory, Greenwich. "Results of Micrometer Measures of Double Stars made with the 28-inch Refractor at the Royal Observatory in the Year 1906."

Royal Observatory, Greenwich. "Proper Motions of Astrographic Reference-Stars from a Comparison with Carrington's Catalogue, 1855, in Zones of N.P.D."

H. F. Newall. "Spectroscopic Observations of Cyanogen in the Solar Atmosphere and in Interplanetary Space."

Royal Observatory, Greenwich. "Observations of Minor Planets from Photographs taken with the 30-inch Reflector of the Thompson Equatorial during the Year 1906."

The following gentlemen were elected Fellows of the Society :—

Harold A. H. Christie, B.A., Flamsteed House, Greenwich, S.E.

Bertram Francis Eardley Keeling, The Observatory, Helwân, near Cairo, Egypt.

Frederick Alexander Lindeman, Sidholme, Sidmouth, Devon.

The following Candidates were proposed for election as Fellows of the Society:—

Henry Boase Austin, J.P., Government Buildings, Bloemfontein, Orange River Colony, South Africa (proposed by R. T. A. Innes).

W. Geoffrey Duffield, Physical Laboratory, Manchester University (proposed by Arthur Schuster).

W. Earnshaw Etzel, B.A., Litt.D., Professor of Science, St. Bernard's, Rochester, N.Y., U.S.A. (proposed by William R. Brooks).

John M. Field, 1 Hart Street, Edinburgh (proposed by William Peck).

James D. Maddrill, Ph.D., International Latitude Observatory, Ukiah, California, U.S.A. (proposed by H. C. Plummer).

William Henry Rees, B.Sc., Mathematical Master, County School, Pontypridd, and 2 Graigeven Place, Pontypridd, South Wales (proposed by Richard Kerr).

Thomas James Forrester Smith, Newstead, Wavertree, near Liverpool (proposed by F. W. Longbottom).

Captain James Weir, F.R.G.S., Examiner of Masters' Mates in Navigation and Seamanship to the Board of Trade, 5 Clive Crescent, Penarth, Glamorgan, South Wales (proposed by Albert Taylor).

ANNUAL GENERAL MEETING OF THE BRITISH ASTRONOMICAL ASSOCIATION.

Wednesday, 1907, October 30.

F. W. Levander, *President*, in the Chair.

The President said that before proceeding to the ordinary business of the Meeting he had a very sad announcement to make. They had since the last Meeting lost one of their Secretaries by death. For some 13 years past, with but few exceptions, Mr. Petrie had been at the President's right hand. He took a great interest in the Association and devoted a considerable amount of his time to its affairs. The Council had passed a resolution expressing their sympathy with Mrs. Petrie, and he was sure that the Meeting generally would wish to be associated with that vote.

The Meeting having sympathetically accepted the resolution, the President said that the Secretaries would write to Mrs. Petrie to that effect.

Capt. Grant read the minutes of the previous Annual General Meeting, which were confirmed and signed.

The names of 9 Candidates for Membership were then read and passed for suspension; and the election by the Council of 4 new Members was unanimously confirmed.

The list of presents received was read, among them being P. Fauth's 'The Moon in Modern Astronomy,' S. W. Burnham's 'Double Stars,' and a part of the Paris Lunar Atlas. The thanks of the Meeting were accorded to the respective donors.

Mr. Hardcastle read the report of the Scrutineers of the ballot for the election of Officers and Members of the Council for the Session 1907–08.

The President said that before asking the Meeting to confirm these appointments he wished to announce that it had been necessary for the Council to appoint a Secretary in the place of the late Mr. Petrie, and Capt. Grant had been elected. This created a vacancy in the list of ordinary Members of the Council, which the Council had filled by the election of Mr. Keatley Moore.

The following were declared duly elected:—*President*: F. W. Levander. *Vice-Presidents*: A. C. D. Crommelin, Sir David Gill, K.C.B., H. P. Hollis, E. W. Maunder. *Treasurer*: W. H. Maw. *Editor*: F. W. Levander. *Librarian*: George Bruford. *Secretaries*: Capt. Frank L. Grant, J. A. Hardcastle. *Other Members of the Council*: G. J. Burns, G. F. Chambers, T. Crawford, H. Ellis, W. T. Lynn, H. K. Moore, H. K. Nield, D. Smart, C. Thwaites, C. T. Whitmell.

The following nomination of Members by the Council as Editor and Directors of the Observing Sections was unanimously confirmed:—*Editor*, F. W. Levander. *Sun*, Rev. A. L. Cortie; *Moon*, Walter Goodacre; *Mercury and Venus*, Henry MacEwen; *Mars*, E. M. Antoniadi; *Jupiter*, Rev. T. E. R. Phillips; *Saturn*, G. M. Seabroke; *Comets*, A. C. D. Crommelin; *Meteors, Auroræ, and Zodiacal Light*, Miss Catharine O. Stevens; *Variable Stars*, Col. E. E. Markwick; *Double Stars*, G. M. Seabroke; *Photography*, F. W. Longbottom.

The Report of the Council on the work of the Session, October 1, 1906, to September 30, 1907, together with the accounts, was next submitted and unanimously adopted, as were votes of thanks to the retiring Members of the Council, the Auditors, and the Scrutineers of the Ballot.

The President then delivered his address. He said that, looking through the addresses of his predecessors, he had found, as might naturally be expected, that the progress of the Association had been a favourite theme. On the present occasion he proposed going further back, to the time when the Association was not, and placing on record in our official publication an account of its origin. The disaster which overtook the (at one time, prosperous) Liverpool Astronomical Society was probably the leading reason for the formation of the British Astronomical Association. The first public indication of a movement in that direction was a letter under date of July 12, 1890, from Mr. W. H. M. Monck in the *English Mechanic*, advocating the formation of an Amateur Astronomers' Association. The following week Capt. Noble (over his usual pen-name of "A Fellow of the Royal Astronomical Society") gave his cordial approval to the suggestion. In the issue of the following week, July 25, the idea was further urged by Miss E. Brown, who had been for five years the Director of the Solar Section of the Liverpool Astronomical Society, and on August 8 Mr. E. W. Maunder announced in the columns of the same paper that a society on the lines suggested by Mr. Monck was actually in course of formation. So cordially was the suggestion of the formation of a new society taken up that in the issue of the *English Mechanic* for August 15, 1890, there appeared a letter from Mr. Maunder containing a draft of the programme of the new society with a Provisional Committee of 36, including some of the best-known professional and amateur astronomers of the country. Matters progressed so rapidly that on October 14 the first General Meeting was held in the hall of the Society of Arts, Adelphi, Mr. Maw acting as temporary Chairman. As the result of the ballot Capt. Noble became the first President. Following the example of the Liverpool Society, the Association appointed Directors of Observing Sections. Most of the Sections were formed from the very first, but those for Solar Spectroscopy and for the observation of Mercury, Venus, Saturn, and Comets were added later.

The Lantern-slide Department, which has proved so successful, was inaugurated at the commencement of 1893. There had been a very gratifying increase in the extent of the Library. When he (the President) undertook the duties of Librarian, in March 1895, it was but a small collection, numbering about 200 books in all, but when his successor came into office last year the number had increased to 2,200. For those they were indebted to the liberality of authors, publishers, and learned societies, as well as to annual grants from the Council. Of the Branches that had been formed, both at home and in Australia, some had, unfortunately, perished of inanition, others had after a time preferred to leave the parent Association and walk alone; others, again, still flourished and were doing good and useful work. At the Meeting held on March 31, 1897, it was announced that the Royal Botanic Society had offered the Association a site in their gardens at a peppercorn for erecting an observatory. The negotiations, however, did not come to a satisfactory issue, and the matter was still in abeyance. The President then detailed the different classes of work open to the Members of the Association, and concluded by reviewing in detail the leading astronomical events of the year.

Mr. Crommelin said he had very much pleasure in moving a hearty vote of thanks to the President, both for his conduct in the Chair during the past year and for the address with which he had just favoured those present. The Association owed a great debt of gratitude to Mr. Levander. As he himself had reminded the Meeting in his Address, he presided over the library from the year 1893 until he assumed the presidential chair, a period of over 10 years, and he had been their Editor since the beginning of the century. These were both very difficult posts to fill satisfactorily; each involved a great deal of tiresome routine work, and on both depended the prosperity of the Association to a considerable degree.

Mr. Maw, in seconding the motion, desired to endorse all that Mr. Crommelin had said as to the great indebtedness of the Association to Mr. Levander for the work he had done ever since the Association had been in existence. It was a particularly happy idea of Mr. Levander's to incorporate in his Address an account of the early history of the Association. As time went on it became increasingly difficult to get anything like an accurate record of the facts associated with the establishment of a society like their own, and Mr. Levander's authentic record, therefore, was not only of very great interest to all Members at the present time, but it would become of increasing historical value as time passed on.

Mr. E. W. Maunder asked to be allowed the privilege of supporting the vote of thanks to the President proposed by Mr. Crommelin. He had known a good deal of the work which Mr. Levander had done for the Association and he wished to heartily endorse what Mr. Crommelin and Mr. Maw had said with regard to the thoroughness and devotion which Mr. Levander had thrown into the work of his several offices. He would like to supplement by a few words what Mr. Levander had said with regard to the origination of the Association. If there was one person more than another to whom it owed its inception it was the late Director of the Solar Section, Miss Elizabeth Brown. She had also been its most munificent supporter. In addition to her subscription she had contributed between £100 and £200 in various gifts, and at her death left them a legacy of £1000.

The resolution was put to the Meeting by Mr. Crommelin and carried with applause.

Mr. Hardcastle read a paper by *Mr. G. D. Hirst* entitled "Some Remarks on Wiring Astronomical Instruments."

This was followed by a paper by the *Rev. T. E. R. Phillips* on "The alleged Disappearance of Saturn's Rings." The writer said that on October 4th the Earth crossed the plane of Saturn's rings, so that those remarkable appendages were presented exactly edgeways to view. Since that date the plane of the rings had passed between the Earth and the Sun, and the side turned slightly towards us—the N. side—had therefore been theoretically invisible. The purpose of his note was to point out that the ring system had never certainly disappeared in his 12¼-in. Calver equatorial, or at any rate it was distinctly visible at the time of writing (October 26) whenever the conditions of seeing were at all good. He was able to observe the planet on the night of October 3rd, when an extremely thin and delicate line of light extended the usual distance on each side of the ball. The following night, the theoretical date of disappearance, the weather was unfavourable. On several subsequent occasions he could not avoid the impression that there was a faint luminosity in the plane of the rings extending for a short distance on each side of the ball. At first he thought the appearance might be an illusion, but on observing the planet more recently, notably on the 19th, 23rd, and 25th October, under very favourable conditions, he had seen unmistakably an extremely thin though apparently continuous line of light extending on each side of the globe for nearly the whole distance of the rings from the planet. As to the explanation, it seemed to him likely that the narrow sharp line observed was due to the Sun shining through the Cassini division and illuminating the edge of B.

Mr. T. Merchant Taylor said that on Friday night (25th October) he was with Mr. Phillips, and they clearly saw the ring as mentioned in the paper. Before that, when using a 9¼-in., he suspected something of the sort, but was not quite sure about the matter.

Mr. Hardcastle asked when we should come on the same side of the plane as the Sun.

Mr. Hollis said that would take place in January.

Mr. Thwaites said that he had failed to see the rings on the 19th with his 5-in. refractor, but he had the opportunity to look through Mr. Newbegin's 9-in. refractor on the following night and three of them saw a very faint indication of the ring on both sides. On a subsequent night he looked with his 5-in. and could not see it, but Mr. Newbegin told him he had again seen it more distinctly with his 9-in.

Mr. Goodacre said he looked on the 4th of October with a 12-in., but could see no trace whatever of the ring, but seeing was not very good.

The President said he was sorry there was not time to read any more of the papers that had been sent in. He wished to announce that Sir David Gill had promised to give an address at the next Meeting. He did not know what the subject would be, but Members might be sure that it would be an extremely interesting one.

The Meeting then adjourned.

ROYAL METEOROLOGICAL SOCIETY.

The first Meeting of the Session was held on the evening of Wednesday, November 20, at the Institution of Civil Engineers, Great George Street, Westminster, *Dr. H. R. Mill*, President, in the Chair.

The Meeting was largely devoted to the consideration of the Reports on the results obtained by the balloon observations made in the British Isles, July 22nd to 27th last. The International Aeronautical Commission has for some years set apart the first Thursday in each month for the ascent of kites and balloons, but at last year's Conference it was decided to make a special effort to obtain information on a series of consecutive days, and the last week in July was finally decided upon for the purpose. Twenty-five balloons with registering instruments were sent up in England and Scotland during the week, under the direction of Mr. W. H. Dines, F.R.S., at Pyrton Hill, Oxon, and at Crinan, on the west coast of Scotland; Mr. J. E. Petavel, F.R.S., at Manchester University; Capt. C. H. Ley, R.E., at Sellack, Herefordshire; and Mr. C. J. P. Cave, at Ditcham Park, Petersfield. Fourteen of the recording instruments have been found. Prof. W. E. Thrift also sent up a number of pilot balloons from Dublin. Nearly all the balloons drifted to the eastward, but several which reached a fair height fell within 20 miles of their starting-point. The heights ranged up to over 12½ miles, the average being about 7½ miles. The records showed that above 7½ miles the temperature remained almost unaltered with change of height.

A paper giving a Discussion of the Meteorological Observations at the British Kite Stations, Session 1906–1907, by *Miss M. White*, *Mr. T. V. Pring*, and *Mr. J. E. Petavel*, *F.R.S.*, was also read. The authors found that the temperature-gradient varies with the direction and the velocity of the wind, and also with the amount of clouds, being greatest for a north-west wind, and on clear and fine days. It appears that the direction of the wind alters at high levels, rotating in a clockwise direction; thus a south wind tends to become more westerly.

Burnham's General Catalogue of Double Stars.

THE Double-star Astronomer, especially if he happens to be located north of 30° N. latitude—and it is nearly a fifty to one probability that he is,—should feel grateful to Messrs. Hussey, Lewis, and Burnham for their recent notable and increasingly weighty contributions to the literature of his subject. Prof. Hussey has given a very complete work on the Otto Struve double stars, and Mr. Lewis has done the same for the elder Struve's double stars. Prof. Burnham's great general catalogue to a certain extent includes the work of the two former astronomers, but not entirely. Indeed it cannot be said to supersede them; because for the Struves' double stars and for many others it is more in the nature of a liberal reference catalogue, whereas the other two works give fairly complete measures and histories of each star and are free from a certain bias.

The convenience of a general catalogue such as Burnham's is so obvious that double-star investigators will welcome it. It is from the hands of a master whose enthusiasm for his branch of science has never been exceeded. One can, indeed, read that in Prof. Burnham's judgment it is nearly a crime for a double-star observer to swerve from his own branch of research; this attitude is hardly to be commended, and it does not possess the virtues which Prof. Burnham's claim implies. We can grant that when an observer finds that he has access to a good telescope, has a keen eye for a double star, and a steady hand for measuring it, he should follow this branch of astronomy if he can profitably do so, but that he should, as it were, have to take an oath of celibacy in doing so is absurd. One can trace the narrowing effect of Prof. Burnham's theory in his own work. To Burnham, a double star is merely two bright points close together which must be measured, and, if possible, an orbit deduced. So the problem presented itself to Burnham some thirty years ago, and so it remains to this day. Thus it comes about that some people style the General Catalogue unimaginative. It is doubtful if modern double-star catalogues maintain the standard of Struve's great folio; perhaps there has even been a regression. Struve had to break the ground so to speak—he had to find the planes of his stars and to determine their magnitudes. The richness and perfection of modern star-catalogues make it easy for the astronomer to identify his stars to-day, and most do so with a scrupulous care; but the modern double-star astronomer prefers to estimate his magnitudes instead of taking them from the authorities. Thus if Argelander has determined the magnitude of a star as 8·0, and the double-star astronomer finds it to be a close double of equal components, it will be found, as a rule, that he calls each 8·0 magnitude, which, as Euclid says, &c. Unless there is reason to disagree with the magnitude found by Argelander, Gould, or Harvard, it should be adopted, and the difference of magnitude of the two components alone estimated. But a graver fault, in which Burnham is followed very generally, is the omission to note colours of double stars. The colours of double stars have certainly been overdone by some astronomers (Smythe occurs to the mind at once), but that is no reason why a simple division should not be used—it is so easy to classify the stars into their three great divisions of white stars (including bluish in most refractors), yellow stars, and reddish stars on the Chandler scale that it should be done.

Prof. Burnham attaches a peculiar importance to things made in the United States, and diminishes in consequence the value of his conclusions. European astronomers have borne and do bear such ungrudging testimony to the great value of the work of their American colleagues, both as mathematical and observational astronomers of the highest class, that to their reading Prof. Burnham's parochial views are curious and amusing. An example occurs in the star β 416, which was really discovered and published some years earlier in Melbourne *, although Prof. Burnham does not say so: this star has been measured at the Cape Observatory, near whose zenith it passes, yet in drawing the apparent orbit the Cape measure is suppressed in favour of others, taken certainly by skilful observers, but at such a poor altitude that the measures must suffer.

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* See 'Reference Catalogue of Southern Double Stars,' p. 172 A.

Prof. Burnham states that his catalogue covers the sky from the N. Pole to 31° S., but we must note that all observers are not treated with republican equality in this respect, and that one more fortunate individual's discoveries are included even if they are 43° S. This is good, but it is so good that it should have been applied to all alike.

We further note that once Prof. Burnham has discovered a double star and named it, the name sticks even if it is erroneous: thus λ Canis Majoris and κ Argûs figure as β stars, although, as pointed out long ago, these are misidentifications, very obviously so in the case of κ Argûs, which does not even rise above the horizon in Chicago. It seems that the principle on which the catalogue has been formed is to enter the double exactly as first reported, even if subsequently error is found: one could not object to this principle if the stars so catalogued were asterisked in some way. Thus Prof. See's useful but somewhat hasty survey resulted in some misidentifications since corrected by other observers, but many of these corrections are ignored.

Although these points do detract from the value of Prof. Burnham's catalogue, its value is incontestable, and astronomers will be grateful to the author and to the Carnegie Institution for arranging its publication. Such a catalogue, which includes every star once entered in a double-star catalogue, raises the question as to what should really be retained as being a double star: both Profs. Hussey and Burnham advocate a liberal policy, and indeed the latter has devoted much time of recent years to measuring neglected very wide pairs. Indeed, some years ago, the writer, who had seen the Cape Astrographic plates, perceived that the day for measuring wide faint pairs with the micrometer had passed. The recent appearance of some of the volumes of the Greenwich and Oxford Astrographic Catalogue enables him to give some proofs of the justness of his views. The samples chosen start with the stars in the Greenwich zone which appear in Burnham's General Catalogue.

No. 22. Espin 113. DM 66° 6. This should be DM 66° 8: the two stars are Gwh. 66°, 49–50, and their positions yield 1894·8, 122°·2, 7″·2. When discovered eight years later by Espin his measure is 122°·0, 6″·8.

No. 29. Espin 114. = Gwh. 66°, 37–38.

1894·8	167°·2	4″·26 Gwh.
1902	161 ·6	5 ·0 Espin.

No. 39. *h* 1940. Anonymous and unmeasured. This is Gwh. 71°, 134–5. The double number shows that both stars are measured at Greenwich.

No. 56. *h* 1941. Anonymous in Gen. Cat. =Gwh. 71°, 145–6.

No. 140. *h* 1952. Not identified in Gwh. Cat.

No. 152. OΣ 6. Companion C. Gwh. 66°, 88.

No. 157. OΣ 7. Companion C. Gwh. 65°, 113.

No. 191. Σ 29. Both components measured at Oxford. Oxf. 31°, 680–1.

No. 195. *h* 1026. Anonymous. =Gwh. 66°, 118–9.

1828	193°·0	8″±	*h.*
1894	190·6	12·2	Gwh.

No. 233. *h* 1973. =Gwh. 71°, 413–4.

1830	44°·9	16″±	*h.*
1895	49·8	17·1	Gwh.

No. 252. *h* 1028. Anonymous. =Gwh. 65°, 92–3.

1828	148°·9	12″±	*h.*
1894	143·9	16·3	Gwh. (180° added).

No. 287. *h* 1983. =Gwh. 71°, 427–8.

1830	306°·7	35″±	*h.*
1895	305·2	41·0	Gwh.

No. 315. *h* 1037. No star in this place in Gwh. Cat.

No. 337. *h* 1040. =Gwh. 65°, 193–4.

1828	356°·4	2″±	*h.* "Delicate."
1894	354·7	5·20	Gwh.

No. 372. Σ 48. 1896·8 335°·8 5″·26 Gwh. (180° added).

No. 409. *h* 1052. Anonymous in Gen. Cat. Is perhaps Gwh. 64°, 257–8. Requires further examination.

No. 442. *h* 1999. Anonymous. =Gwh. 69°, 367–8,

1896·3	16°·3	25″·6	Gwh.

No other measure.

No. 450. *h* 1055, 8″±. Anonymous. Place is closely that of Gwh. 64°, 273, which is single.

No. 460. A.G. 8. =Gwh. 68°, 285–6.

1898·1	215°·2	17″·8	β.
1893·8	213·4	17·4	Gwh.

No. 465. A.G. 9. =Gwh. 68°, 314–5.

1891·8	71°·3	6″·6	β.
1893·8	68·9	7·1	Gwh.

No. 512. *h* 1059. Anonymous. No star here in Gwh. Cat.

No. 515. *h* 1061. Anonymous. =Gwh. 66°, 352–3. *h*'s distance is too small.

No. 649. *h* 2028. This pair is separately observed in several meridian catalogues.

No. 659. *h* 1057. Ditto. =Gwh. 67°, 396–7.

No. 674. *h* 1077. Anonymous. =Gwh. 70°, 962–3.

No. 675. The ♅ double star S. p. =Gwh. 65°, 415–6.

No. 797. Σ 130. =Gwh. 69°, 710–1.

1832·1	187°·7	7″·49	Σ.
1895·9	185 ·9	7 ·84	Gwh.

So much for Greenwich plates. The Oxford stars were only looked for at about 6^h R.A., so as to save the mental trouble of the correction for precession when identifying.

No. 3161. *h* 5468. No companion in Oxf. Cat.

No. 3166. *h* 379. =Oxf. 31°, 15456–7.

1820	130°±	5½″±	*h*.
1903	112 ·5	9 ·4	Oxf.

No. 3193. OΣ (App.) 69. =Oxf. 31°, 15352–3.

No. 3203. Σ 862, 6″·5. =Oxf. 30°, 14054–5.

No. 3247. Σ 879, 8″·3. =Oxf. 30°, 14211–2.

No. 3363. *h* 388. Anonymous and unmeasured. =Oxf. 30°, [14816–7.

No. 3377. Σ 907. =Oxf. 30°, 14987–8.

No. 3384. A.G. 113. =Oxf. 31°, 16352–3.

No. 3399. A.G. 115, 4″. =Oxf. 30°, 15716–7.

No. 3433. *h* 730. Anonymous and unmeasured. =Oxf. 30°, [15491–2.

No. 3508. =Oxf. 31°, 17008–9.

1825·2	25°·9	80″·7	S.
1903·2	26 ·1	81 ·9	Oxf.

No. 3706. =Oxf. 30°, 16942–3.

1820	5°±	6″±	*h*.
1903	3 ·4	10 ·2	Oxf.

These few examples are sufficient to show the class of double star which does not require the attention of the micrometer at the telescope. If a century hence, when the plates are repeated, considerable change is shown in some pairs, it may then be worth while to put such pairs on the observing list. In the meantime there is better work to be done.

Prof. Burnham's success and enthusiasm have encouraged other Americans to pursue double-star astronomy. His most successful followers are Profs. Hussey and Aitken, but the former seems now to have abandoned the field. These two astronomers have worked on a system such that each star in the Durchmusterung will be examined in due course. The results of this work must be most valuable from a statistical point of view, but it is slow and arduous. The great number of new double stars already found by Profs. Hussey and Aitken proves the advantage, at least now-a-days, of a systematic plan. So few orbits of double stars are known to-day that no generalizations can be founded on them, but all this will be altered in a century or two. It is the writer's belief that the comparison which then can be made between the double stars of the Milky Way and those of the yellow stars will be an important factor in the solution of the sidereal problem.

The second part of Prof. Burnham's great work presents an enormous amount of compilation and research. A large number of proper motions are given with an opinion as to the nature of the motion. The Author is especially and properly severe on the accumulation of useless measures of certain fashionable stars to the neglect of others. He further loses no opportunity of pointing out the uselessness of attempting orbits until one revolution of the components has taken place.

Prof. Burnham often draws a curious distinction: thus with Antares we learn that the companion was "discovered" in 1845 (in America), but had been "seen" in India in 1844 and in Europe in 1819!

The following errata have been noticed in Part I.:—

No. 2292. *For* G. *read* Z.
7941. " 34′ *read* 24′.
8443. " 9 mins. *read* 8 mins.
8647. " $28^{\mathrm{m}}\ 52^{\mathrm{s}}$ *read* $30^{\mathrm{m}}\ 3^{\mathrm{s}}$.

R. T. A. I.

The Sun-spots of 1907 *August.*

The chief sun-spot group of August 1907 was seen during the closing days of the month. The group—No. 6241—was not then a new formation; it had sprung up originally, as Group 6231, on August 5, not far from the centre of the disc. It then consisted of a number of quite small spots scattered over a considerable area. These tended to enlarge and to come together, and before the group passed out of sight at the west limb on August 13 it had assumed very nearly the typical appearance of a "stream" of spots, and when last seen appeared as two large composite spots. It was next seen on August 27 at the east limb, having grown considerably during the interval. It was now a magnificent stream, easily visible to the naked eye on August 29 and the succeeding days.

Only one other group of the first rank was seen during the month. This was Group 6236; like Group 6241 a stream formation, but longer and less compact, and so not as readily visible to the naked eye. It took its rise in the unseen hemisphere.

The other groups call for little notice. There was a fair amount of steady activity throughout the month, four or five groups being always visible at any one time, but none, except those already mentioned, were of remarkable size or appearance.

Rotation 721 began 1907 August 18^{d}·060.

Spot-Groups in 1907 August.

No. of Group.	Observed. From	Observed. To	Duration.	Date for C.M.	Longitude.	Latitude.	Remarks.
	1907.	1907.	days.	d	°	°	
6228	Aug. 1	Aug. 2	2	July 30·8	241	−17	Pair of small spots.
9	4	4	1	Aug. 5·4	167	−10	Pair of small spots.
6230	5	7	3	2·7	203	+ 3	Irregular stream.
1	5	13	9	7·0	146	+ 8	Two composite spots.
2	5	13	9	11·2	90	−15	Regular spot.
3	9	19	11	13·7	57	−20	**Return.** Regular.
4	10	21	12	16·0	27	+15	Regular spot.
5	13	16	4	11·0	93	+19	Pair of small spots.
6	14	15	12	20·1	333	−25	Very fine stream.
7	15	18	4	13·6	59	−13	Small spot *n*. Gr. 6233.
8	16	26	11	21·8	311	− 9	**Return.** Regular.
9	19	29	11	25·2	266	−16	Regular spot.
6240	22	29	4	25·2	266	−10	Intermittent.
1	27	Sept. 8	13	Sept. 2·7	154	+ 7	**Return.** Very fine stream.
2	28	Aug. 29	2	Aug. 24·0	282	−18	Small spot.
3	31	Sept. 9	10	Sept. 4·2	134	−18	Sparse stream.

Notes on the Principal Spot-Groups.

Group 6230.—Pair of small spots developing into an irregular stream. Area on August 6 about 100.

Group 6231.—A number of small spots irregularly scattered, which combine to form two composite spots. Area on August 9 about 400.

Group 6232.—A regular spot. Area on August 9 about 100.

Group 6233.—Return of Group 6215. A regular spot. Area on August 13 about 120.
Group 6234.—A regular spot. Area on August 14 about 150.
Group 6236.—A very fine long, straight stream. Area on August 19 about 700.
Group 6238.—Return of Group 6220. A regular spot. Area on August 20 about 100.
Group 6239.—A large regular spot. Area on August 24 about 250.
Group 6241.—Return of Group 6231. A very fine continuous stream. Area on August 31 about 1200.

E. Walter Maunder.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Copernicanism and the Church.

Gentlemen,—

In your issue of this month Father Cortie, in criticizing some remarks of Prof. R. T. Crawford, says:—" It was never a dogma of the Church that the Earth was stationary, and consequently to think the contrary was not only not the greatest heresy, but no heresy at all."

It is difficult to believe that Father Cortie has recently read the official records of Galileo's two trials. The important documents have been published several times. I quote, for convenience, from Mr. Fahie's book on Galileo. In the trial of 1615–16 the Qualifiers of the Holy Office were asked to report on the two doctrines: (1) that the Sun is the centre of the world and therefore immovable from its place; (2) that the Earth is not the centre of the world and is not immovable, but moves, and also with a diurnal motion. On February 24, 1616, they reported that the first doctrine is "false and absurd philosophically, and formally heretical, inasmuch as it expressly contradicts the doctrines of Holy Scripture," and that the second deserves the like censure in philosophy and as regards its theological aspect is at least "erroneous in faith." Two days afterwards Galileo was ordered by the Commissary-General of the Holy Office, "in the name of His Holiness the Pope and the whole Congregation of the Holy Office," to relinquish altogether the said opinion. A few days later, a decree of the Congregation of the Index was published by order of the Pope, suspending or prohibiting three books (including the 'De Revolutionibus' of Copernicus) for teaching the "Pythagorean doctrine (which is false and altogether opposed to Holy Scripture) of the motion of the Earth and the quiescence of the Sun."

In the sentence which terminated the second trial before the

Holy Office (June 22, 1633), after repeated references to the belief that the Earth moves as "false," Galileo is stated to have rendered himself vehemently suspected of "heresy, that is, of having believed and held the doctrine .... that the Earth does move," and he is absolved from various penalties provided that he abjures "the said errors and heresies."

The sentence is a very long one, and refers at some length to Galileo's disobedience to the orders given to him in 1616, but I can find nothing in it suggesting the "amateur exegesis" to which Father Cortie refers. It is difficult to decide at this date what were the determining causes which led the Holy Office to interfere with Galileo; it is likely enough that if he had made fewer enemies by his caustic pen and tongue, and not attempted "amateur exegesis," he would never have been *tried*, but he was undoubtedly *condemned* for purely astronomical opinions, aggravated by disobedience.

It is not for me to discuss with Father Cortie the technical meanings of "dogma of the Church" and "heresy," nor are the pages of the *Observatory* suitable for such a discussion. I merely note that he and the Holy Office of 1633 differ as to whether the motion of the Earth is "heresy." A lay astronomer like Prof. Crawford may well be pardoned if he attributes to the "Church" generally the condemnation so emphatically pronounced by two of its most important agencies, the Holy Office and the Congregation of the Index.

Yours faithfully,

ARTHUR BERRY.

King's College, Cambridge,
1907, Nov. 16.

Early Copernicans.

GENTLEMEN,—

It has often been stated that Newton was born the same year in which Galileo died. Thus, in the life of the latter given in the 'Encyclopædia Britannica,' we read (vol. x. p. 34):—"The coincidence of the day of his birth with that of Michel Angelo's death was paralleled by the coincidence of the year of his death with that of the birth of Isaac Newton."

This latter supposed coincidence, however, is founded on a confusion between the two styles of the calendar then observed in England and on the Continent. Galileo died on the 6th of January, 1642, by the Gregorian or New Style; Newton was born on the 25th of December, 1642, by the Julian or Old Style, which would correspond to the 4th of January, 1643, by New Style. Horrox, the great pride of English astronomy at the time, died very nearly a complete year before Galileo; Crabtree and Gascoigne probably both came to an early death in 1644, the latter at the battle of Marston Moor, fought on the 2nd of July. Two years after that, in 1646, Flamsteed was born. Both Newton and Flamsteed were born during the civil war in England (Newton just after it had begun), and whilst Germany was being desolated by the thirty-years' war, which did not terminate until 1648. It is refreshing to remember that it was during these times of trouble in England that in the year 1645 those meetings were held of what Boyle called the *invisible* college, which developed, when quiet was restored to the country, into the Royal Society. One of the most diligent promoters of these meetings was John Wilkins, who was the first secretary of the Society when it was incorporated in 1662, and became Bishop of Chester in 1668.

I should like here to quote a passage at the end of Dr. Dreyer's most valuable recent work on the 'History of Planetary Systems,' which must surely have a place, and a prominent one, in every astronomical library. He says:—

"An interesting proof of the gradual change of feeling as regards the Earth's motion is afforded by the utterances of some prominent men in England in the beginning and middle of the century. In several places in his writings Francis Bacon speaks of the Copernican system without in any way doing justice to it, and as if it were of no greater authority than the notion of the Ionians that the planets describe spirals from east to west. On the other hand, we find that John Wilkins, afterwards a brother-in-law of Cromwell, and still later Bishop of Chester, in 1640 published a 'Discourse concerning a New Planet, tending to prove that (it is probable) our earth is one of the planets'; while Milton, in the 'Paradise Lost,' speaks sympathetically about the new system."

Milton, we know, had seen and conversed with Galileo. His somewhat hesitating views respecting astronomical systems are set forth in detail in Mr. Orchard's interesting work 'Astronomy in Paradise Lost.' One passage is well known; the question

> "How nature, wise and frugal, could commit
> Such disproportions . . . . . ."

as would be implied in the Ptolemaic system which so long held sway in the world.

But there is no hesitancy about Wilkins's acceptance of the Copernican system, which is most enthusiastic. The work to which Dr. Dreyer alludes first appeared as a second part of the 'Discovery of a world in the Moone,' the first edition of which was published in 1638. He there expresses a belief in the habitability of the Moon, agreeing with the views expressed in 'The Man in the Moone,' by Francis Godwin, Bishop of Hereford (also a Copernican), which was published after his death in 1633. But those were very early days of selenography, less than a quarter of a century after the "Tuscan artist" had first directed a telescope to our satellite.

Wilkins was born at Fawsley, in Northamptonshire, in 1614, took his master's degree at Oxford in 1634, and became Warden of Wadham College in 1648. He took the Parliamentary side in the Civil War, and married a sister of Cromwell (then a widow named French) in 1656. Nevertheless, he obtained preferment after the restoration, became Prebendary of York in 1660, and Bishop of Chester in 1668. As such, and indeed in every capacity, he did his utmost to promote peace and goodwill all round in very troublous times, and Archbishop Tillotson says of him:—"There are or have been few in this age and nation so well known and greatly esteemed and favoured by so many persons of high rank and quality, and of singular worth and eminence in all the learned professions." He died in 1672, the year in which Newton was elected into the Royal Society, and three years before the foundation of the Royal Observatory, Greenwich.

Yours faithfully,

Blackheath, 1907, Nov. 6. W. T. LYNN.

Some Counsels of Perfection.

GENTLEMEN,—

Will you allow me space for some remarks on a paper in your current number entitled "Some Counsels of Perfection"? Of course "Counsels of Perfection" are not expected to be immediately attainable in this imperfect state of Society: it is in the hope that my comments may aid the writer's object that I address you.

I have for many years been a strong advocate of the Metric System. Of course the Mètre must be considered an arbitrary unit, but the advantage of the connection of the units of Length, Weight, and Content are great, and the nomenclature offers a number of secondary units in abundance. I do not agree with the writer that in this the system has failed; as a matter of fact though the astronomer may have met mainly with the ratio 1 : 1000 in his reading and practice, yet the other units are used freely. The Decimètre and Centimètre are (especially the latter) for common use by those who require units between the Mètre and Millimètre. The Decigramme and Hectogramme are freely used, so, too, the Hectolitre and the Hectare. On the other hand the Millilitre is commonly known as the Cubic Centimètre. One thousand kilos it is true are a Tonne, but then 100 are known as a Quintal. That all the units are not everywhere used, but selections, is no objection: we do not (all of us) use all the words in an English dictionary, but that is not a disuse of the language.

As regards Circular Measure. In face of the recent publication of Tables of the Logarithms of the Circular Functions by the Bureau de Guerre, and the use of the Centesimal division of the Quadrant in their maps of France, it is too much to say that that Division is exploded. When tables of logarithms of more than seven places are required these French Tables are employed. If I recollect right, some calculations required by Sir David Gill were executed in Paris with these tables, there being no others available.

For my own part I have always felt that the originators of the Metric System made a mistake in adopting the Quadrant as the Fundamental unit of circular measure.

The Circumference is a natural unit, the Quadrant is not; therefore I think the former and not the latter should have been taken as Fundamental. Were it so, and were we to divide the sidereal day decimally (clocks could be easily made to do this), we should have a much more convenient arrangement than now and many errors would be avoided.

The writer's proposals seem to me to have one sole advantage if they could be carried out. Tables of seven-figure logarithmic Circular Functions would be easier to interpolate with than those of Bruhns, &c., and less bulky than Taylor, Bagay, or even Shortrede. From what I have said, however, it will be, I think, evident that it is hardly worth while to go to the expense of calculating and printing new seven-figure tables.

I would strongly advise any computer to use Shortrede. I have used no other book (voluntarily) for some 50 years and more. Many years ago I gave a copy to the late Mr. Stone, who told me that he preferred them to any others. I know there is a prejudice against the form of the figures, but to me they were* clear. They are said to be crowded, but the problem of including the matter in the space forced compression. They are absurdly dear: Bruhns' seven-figure tables with the same amount nearly of matter are sold (with English preface, &c.) at a sixth of the price, and with good management Shortrede should be much cheaper.

I have trespassed, I fear, too much on your space, and remain,

Maida Hill, W., Yours faithfully,
1907, Nov. 11. J. F. TENNANT.

Linné under High Illumination.

GENTLEMEN,—

In Prof. W. H. Pickering's article, to which I referred in the *Observatory* for October, pp. 384–386, he makes mention of the evidence given by old photographs as to the change that has taken place in Linné. I do not know that anyone had previously thought of making an examination of old photographs to see if they could throw any light on its change. Possibly astronomers had not thought it worth while to do so unless the photographs were taken near sunrise or sunset, and no doubt previous to the change photographs were not good enough to show sufficient detail at such times; but Prof. Pickering has found that the photographs of its appearance when the Sun is at a good altitude above it are sufficient to show a change, the white spot that marked its place then, as now, having evidently grown smaller. Among the photographs he examined, some stereographs taken by the late Samuel Fry at Brighton are not included; and as I happen to possess copies of these I have been examining them. They are undated, as is also the accompanying explanatory pamphlet (published by Horne and Thornthwaite), but I hear from his son that they were made about 1858 or 1859. I have two copies, but one, unfortunately, is broken, and only a fragment remains. I call this No. 1. The perfect stereograph consists of one photograph taken before full (perhaps one day before), which I call No. 2, and of another taken two or three days after full, which is hardly definite enough to be of value in this investigation. In No. 2 there is a defective line across the photograph which may affect Bessel, and the spots called in Prof. Pickering's paper SG, SGA, and *x*, though I am not sure whether it does. No. 1 seems to be taken from identically the same negative as No. 2 is from, but it has not the flaw just mentioned.

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* My eyes are old and the pages are browned by years of use: both should be on the retired list.

Prof. Pickering, in his article already alluded to ('Harvard Annals,' vol. xxxii.), gives (plate 9) copies of two photographs—fig. 1 taken in 1857, and fig. 2 in 1894. I have compared these with Fry's photographs, and also the Map 5C in the Harvard Atlas of the Moon ('Harvard Annals,' vol. li.), and tabulate the results below:—

Photograph.	Date.	More conspicuous than	About equal to	Less conspicuous than
Harvard, fig. 1 ...	1857	α, β, γ, δ		
Fry, No. 2	1858 or '9	α, β, γ, δ		
Harvard, fig. 2 ...	1894	β, γ	......	δ
" 5 C ...	1901	α, β	γ	δ

Harvard, fig. 1 ...	1857	B, SG, SGA	......	*x*
Fry, No. 1	1858 or '9	SG, SGA	......	*x*
" No. 2	"	SG, SGA	*x*	B
Harvard, fig. 2 ...	1894	......	......	B, SG, SGA, *x*
" 5 C ...	1901	......	B, SGA	SG, *x*

In the first table the spots I have named α, β, γ, and δ are four bright spots on Mare Serenitatis nearly in a line with Bessel, α being the southernmost one. They are called on the British Association map of the Moon $1\,E^{\epsilon 1}$, $1\,E^{\epsilon 7}$, $1\,E^{\epsilon 3}$ (Posid. γ), and $1\,F^{\zeta 3}$.

In the second table, B signifies Bessel, SG Sulpicius Gallus, SGA Sulpicius Gallus A, and *x* is the name which Prof. Pickering gives to the conspicuous crater between SG and the corner of Mare Serenitatis. Bessel is more difficult to judge of than the others, on account of the bright light which extends round it to some distance. It will thus be seen that Fry's photographs confirm those described by Pickering in showing a decrease in the conspicuousness of Linné as a white spot since 1858.

West Hendon House, Sunderland,
1907, Nov. 23.

Yours truly,
T. W. Backhouse.

Leeds Astronomical Society.

Gentlemen,—

As in the very kind review of the above Society's Journal for 1906 (pp. 428–9 of the November *Observatory*) no mention is made of Mr. Roebuck's very interesting paper "Jai Singh and his Indian Observations," a paper illustrated by twelve photographs and diagrams, generously provided by the author, I hope I may be allowed this opportunity of drawing attention to it, as the information contained in it is not easily accessible elsewhere. The publishers of the Journal are W. Wesley and Son.

Yours faithfully,
Leeds, 1907, Nov. 8.
C. T. Whitmell.

NOTES.

Comet Notes.—M. Kamenskij and Mdlle. E. Korolikov have calculated the perturbations of Encke's Comet, and an ephemeris for the apparition of 1908. The following are the elements for the respective apparitions:—

T	1905 Jan. 11·84 G.M.T.		1908 Apr. 30·14 G.M.T.	
φ	57° 54′ 20″		57° 56′ 57″	
☊ ..	334 26 49	1904·0.	334 30 18	1908·0.
π	159 2 27		159 5 39	
i	12 35 37		12 36 41	
n	1075″·46		1076″·46	
log q ..	9·5295		9·5283	

Ephemeris for Berlin Noon.

	R.A.			N. Dec.			R.A.			N. Dec.	
	h	m	s	°	′		h	m	s	°	′
Jan. 3....	23	1	30	1	46	Feb. 20	0	3	51	7	44
7....	23	5	0	2	2	24	0	11	9	8	28
11....	23	8	49	2	22	28	0	18	51	9	15
15....	23	12	57	2	44	Mar. 3	0	27	0	10	4
19....	23	17	23	3	8	7	0	35	35	10	56
23....	23	22	7	3	34	11	0	44	43	11	50
27....	23	27	9	4	3	15	0	54	24	12	46
31....	23	32	29	4	34	19	1	4	44	13	44
Feb. 4....	23	38	7	5	7	23	1	15	49	14	43
8....	23	44	4	5	43	27	1	27	44	15	45
12....	23	50	20	6	21	31	1	40	36	16	47
16....	23	56	55	7	1	Apr. 4	1	54	36	17	49

The comet will then remain near the Sun for some time; in June and July it will be well placed for southern observers. It will be brightest on May 9, and nearest the Earth (distance 0·328) on June 15.

Comet *d* 1907 (Daniel) has been again picked up by Hartwig at Bamberg, Nov. 5, mag. 8·5; it will be fairly well placed in the morning sky in December. The following ephemeris (for Berlin midnight) is by Kritzinger (*Ast. Nach.* 4208):—

	R.A. h m s	S. Dec. ° ′
Dec. 3	14 8 55	7 39
9	14 18 3	8 8
15	14 26 27	8 32
21	14 34 8	8 51
Dec. 27	14 41 5	9 5
Jan. 2	14 47 21	9 15
8	14 52 56	9 20

The following elements and ephemeris (for Berlin midnight) of Comet *e* 1907 (Mellish) are by M. Ebell (*Ast. Nach.* 4212):—

T	1907 Sept. 14·402 Berlin M.T.	
ω	294° 22′·6	1907·0.
☊	54 35·5	
i	119 37·4	
log *q*	9·99254	

	R.A. h m s	N. Dec. ° ′	Brightness.
Dec. 3........	0 36 16	27 48	0·51
7........	0 22 17	27 24	0·37
11........	0 12 13	27 3	0·28
15........	0 5 11	26 49	0·22
19........	0 2 51	26 42	0·18

Several observations were obtained during November, but the comet was faint and ill-defined.

Ast. Nach. 4210 contains an article by A. Kopff on the motion of the tail-material of the Comet 1903 IV. His principal conclusion is that the tail deviated notably from the plane of the comet's orbit, an effect which could scarcely be produced by solar action alone.

A. C. D. C.

MINOR PLANET NOTES.—The following new planets have been found:—

By Palisa: AU, Nov. 4. (This seems to have been a visual discovery, the first after a long interval.)

By Kopff: AN, Oct. 4; AO, Nov. 1; AP, AQ, AR, AS, AT, Nov. 3; AV, Nov. 4; AY, AZ, Nov. 6.

By Scheifele: AW, Nov. 4; AX, Nov. 5; BA, Nov. 7.

By Wolf: BB, BC, BD, Nov. 8.

AO was at first supposed to be 320 Katharina; the planet AQ is identical with 528 Rezia; AX, BA are probably 518 Halawe and 566 Stereoscopia.

The Jovian planet 617 Patroclus is now in opposition (mag. 12·5) and has been reobserved; the following elements are by Vladimir Heinrich (*Ast. Nach.* 4212):—

Epoch and Osc. 1907 Dec. 14·0 Berlin M.T.

M	73° 1′ 25″	
ω	302 25 48	1910·0.
☊	43 28 36	
i	22 3 15	
φ	8 14 38	
μ	300″·532	
log a	0·714644	

Corrected Ephemeris (Berlin Midnight).

	R.A. h	m	s	N. Dec. °	′
Dec. 3	4	36	46	36	0
9	4	32	27	36	4
15	4	28	14	36	5
Dec. 21	4	24	15	36	4
27	4	20	37	36	1

A. C. D. C.

THE TRANSIT OF MERCURY.—The observation of this phenomenon on November 14 was much impeded by clouds in England generally, and at Greenwich the planet was seen on the Sun for a few seconds only at a time, once or twice during the morning when the clouds broke. In France they seem to have been more fortunate. Prof. Moye writes to us from Montpellier that he saw the transit in a fine sky, but the seeing was somewhat unsteady and the Sun's limb was boiling. He saw no sign of a luminous ring at ingress or egress, nor any halo during transit. The planet appeared perfectly round, except that the black ligament was seen for fully a minute.

The number of the *Comptes Rendus* for November 18 contains reports of the phenomenon from various observatories. At the Nice Observatory it appears to have been especially well seen, for the times of contact were observed with four instruments, the diameter of the planet measured micrometrically by the same number, and the transits of the Sun and Mercury across the meridian were observed with the transit instrument. Two observers saw a ligament or black drop for some seconds at second and third contact; and one noted a whitish illumination around the planet just before third contact, whilst another noted yellowish surroundings. Nobody appears to have seen a brilliant halo except observers at the observatory at Bourges, and they give reasons for thinking the appearance was subjective; but M. Borelly and other observers at Marseilles saw Mercury, which is said to have been of a violet hue, surrounded by a greyish nebulous ring. The observer named also believes he saw Mercury for a few seconds an hour before first contact.

Count de la Baume Pluvinel, at the invitation of General Bassot, set up two spectroscopes at the Nice Observatory. Among other observations he looked for illumination round the part of Mercury's disc exterior to the Sun, but this was not visible.

SATURN'S RINGS.—The sensational telegram mentioned on the last page of this number appears to be based on the observation of bright knots on Saturn's rings (see November number, p. 428), and may have been induced by the following paragraph written by Clerk-Maxwell in summing-up his investigation of this subject:—

> We conclude, therefore, that the rings must consist of disconnected particles; these may be either solid or liquid, but they must be independent. The entire system of rings must therefore consist either of a series of many concentric rings, each moving with its own velocity and having its own system of waves, or else of a confused multitude of revolving particles not arranged in rings and continually coming into collision with each other. Taking the first case, we found that in an indefinite number of possible cases the mutual perturbations of two rings stable in themselves might mount up in time to a destructive magnitude, and that such cases must continually occur in an extensive system like that of Saturn, the only retarding cause being the possible irregularity of the rings.
>
> The result of long continued disturbance was found to be the spreading out of the rings in breadth, the outer rings pressing outward, while the inner rings press inward.

(Darwin's 'Tides,' p. 331.)

With reference to some remarks made at the Meeting of the R. A. S. reported in this number, it may be noted that several continental observers announce observations of the rings in the early days of November.

AN ASTRONOMICAL DIRECTORY.—Brief mention was made in our September number (p. 370) of a useful book, a list of observatories and astronomers, recently issued from the Belgian Observatory. Totalling the entries in the index we find that there are about 1560 astronomers and 500 observatories. We fancy this number of astronomers errs in the small direction: we call to mind several names not in the list which might be there. In the list of observatories, too, there are some of small resources, whilst other larger establishments are omitted. But it must be very difficult to make a perfect list of this kind, and for this we can only repeat the praise given in the former note.

MESSRS. PHILIPS have sent for our inspection a Standard Time Dial designed by Prof. R. A. Gregory, F.R.A.S., of Queen's College, London. The apparatus consists of a stout wooden board having on each side a printed clock-face showing the hours from 1 to 12 A.M. and P.M. Within the circles formed by the numbers are rotating discs—one bearing a map of the northern hemisphere, and the other that of the southern. By a simple adjustment the standard time at any place can be found which corresponds to any particular Greenwich time and *vice versa.* Price 3*s.* 6*d.*

ROYAL INSTITUTION.—A Christmas Course of Illustrated Lectures, adapted to a Juvenile Auditory, will be delivered at the Royal Institution by Sir David Gill, K.C.B., LL.D., D.Sc., F.R.S., on "Astronomy Old and New." The dates of the Lectures are December 28 (Saturday), 31, 1907; January 2, 4, 7, and 9, 1908, at three o'clock.

THE Lalande Prize of the Institute of France has been awarded to Mr. Thomas Lewis, of the Royal Observatory, Greenwich, and Secretary of the Royal Astronomical Society. Readers of this magazine will no doubt congratulate Mr. Lewis, and will feel glad that this prize has once more been given to an Englishman, the last occasion when this happened being in 1880, when Mr. E. J. Stone received the Lalande Prize for his Cape General Catalogue. During the last twenty years there have been twenty-one recipients, of whom nine were American, nine French, one South American, one Italian, and (the present award) one Englishman. H. P. HOLLIS.

M. STEPHAN is about to retire from the Directorship of the Observatory of Marseilles. The selection of the occupant of such posts is entrusted to the Academy of Sciences, who select two candidates to be presented to the Minister of Public Instruction, the name of them being indicated as preferable. The selected candidates for Marseilles are M. Bourget and M. Simonin (given in this order). The candidates similarly submitted for the Directorship of the Algiers Observatory, vacant by the death of M. Trépied, are M. Gonnessiat and M. Fabry.

PROF. J. C. KAPTEYN, of the University of Gröningen, will hereafter spend several months of each year at the Mount Wilson Observatory, and take charge of such parallax and other similar work as may be done in connection with his "Plan of Selected Areas." So far as possible, the working programme of the 60-inch reflector will be arranged so that the photographs, both direct and spectroscopic, will be of service for Prof. Kapteyn's studies of stellar distribution, as well as for the prime purpose of the Observatory—the investigation of stellar evolution (Publications Ast. Soc. Pac., No. 116).

DR. STRÖMGREN, a Tutor of Kiel University, has been appointed Director of the Copenhagen Observatory, in succession to Prof. Thiele, retired.

SIR GEORGE DARWIN is the only astronomer on the Council of the Royal Society for the ensuing year.

A MAGNETIC observatory in connection with the work of the National Physical Laboratory is being established at Eskdalemuir, Dumfries. Mr. G. W. Walker, of Trinity College, Cambridge, and Assistant to the Professor of Natural Philosophy in the University of Glasgow, has been appointed Superintendent.

WE have received from the publishers, Messrs. Bagster & Sons, Paternoster Row, a copy of the new edition, the eleventh, of Mr. W. T. Lynn's 'Celestial Motions.' Our readers, who know the author so well by his writings, need not be told that this book is carefully written, nor that it is full of information. There is not too much information, but just enough. This is a small book of a hundred and twenty pages, which we can sincerely recommend as a useful reference book giving concisely many facts of astronomy that one often has occasion to look up. (Price two shillings.)

MR. F. K. McCLEAN, of Rusthall House, Tunbridge Wells, left England early in November for the purpose of observing the Total Solar Eclipse of January 3 next. He purposed going to Sydney by the ordinary route, and had chartered a steamship to take him from there to Flint Island.

THE next Meeting of the Royal Astronomical Society will be on Friday, December 13; of the British Astronomical Association on December 18.

FROM AN OXFORD NOTE-BOOK.

A BUNDLE of researches has just come to hand showing the varied and constant activity of the last years of M. Lœwy. It was remarked last month that he had felt himself in sore need of rest, and that his sudden death was doubtless due to the need being even more urgent than he realized. An incident has been related to me which accentuates this tragic view, and which his many friends will read with deep sympathy. During the morning of the day of his death he was showing his photographs of the Moon to a number of scientific colleagues, and the following words broke from him and arrested such attention that they were noted down by one of those present:—

> Travailler comme si on devait vivre éternellement et se hâter comme si on ne devait pas avoir de lendemain.

He died at 4 P.M. that afternoon—fell back suddenly into the arms of a friend while making a speech.

There is no doubt that the great and rapid development of astronomy in recent years has severely taxed the strength of many who have no means of escape from the increase of work.

Are the words he uttered quoted from somewhere? and can any reader kindly give the reference?

Signs of increased astronomical activity in Canada are particularly welcome. The Report of the Chief Astronomer for the year ending 1905, June 30, tells us of the imminent completion of the new Dominion Observatory at Ottawa. Apparently the old building at 26 Wellington Street, which had been occupied for nine years, was vacated at Easter 1905, and the staff moved into the new one with great satisfaction. This is perhaps rather old news by now, as we are reminded by the contiguous records of the abortive eclipse expedition of 1905 to Nova Scotia. Bad luck, however, is not likely to be persistent, and we shall look forward to more positive results in the next volume. Meanwhile the journals of the Royal Astronomical Society of Canada testify independently to the awakening of activity. The Society was fortunate in obtaining an Address from Prof. Rutherford before he left Canada, on "Some Cosmical Aspects of Radioactivity" (Vol. I. No. 3), the value of an otherwise excellent summary being enhanced by the knowledge that it is first-hand. We learn from the same number that "the new spectrograph is now in regular use" at the Dominion Observatory, "and gives star-spectra of fine quality for measurement"; and there is an interesting note on the "Tides of Lake Huron." In the latter, at the foot of p. 183, occurs the phrase "Although the curve is disturbed by *seiches* movements" &c. Does this mean that the *seiches* are not being studied? It seems almost to imply that they are regarded as inconveniences merely, whereas a more important position might be claimed for them. From the next number of the Journal we learn that "seven spectroscopic binaries are under observation, and the measurements on some of these are fairly well up to date." The staff has been materially strengthened by the appointment of three new assistants. And it appears that some excellent photographs were obtained of Daniel's Comet. What is the best way of securing a comparison of the various photographs of this comet, in order to trace possible changes in the tail within a few hours? The opportunity for obtaining a picture of it at any one observatory was short, and such comparisons can only be made by collecting results from different observatories. Yet this means much expense in interchanging copies unless the times are first tabulated. In response to some remark of this kind Prof. Barnard very kindly sent a list of the dates of his photographs, which may be given here as possibly of use to others:—

BARNARD'S DATES OF PHOTOGRAPHS OF COMET DANIEL.

June 10; July 3, 11, 13, 15, 17, 18, 19, 20, 29, 31; August 1, 3, 5, 6, 8, 9, 10, 11, 12, 13, 14, 17, 19, 20, 21, 22, 23, 24, 25, 29, 30, 31; September 2, 5, 8, 11, 12.

A thoroughly useful piece of indexing is worthy of special remark; for there may be others who, like the writer, have often felt the need of it, and spent time which might have been saved by it, and yet may not know of its existence. Pages 71 to 74 of volume lv. part I of the 'Harvard Annals' contain a table for converting the name of a variable star into its number, in the manner best explained by giving a portion of the table:

Constellation.	R.	S.	T.	&c.
Andromeda	001838	003740	001726	&c.
„ R.......	004533	235048	R	
„ S.......	....	230752		

Thus S Andromedæ is 003740; SS Andromedæ is 230752; for RT Andromedæ we are referred by the letter R to a note following the table, and so on. The numbers are those of Prof. Pickering's system, the first two digits giving the hour, and the next two the minutes of R.A. for 1900: the last two giving the degrees of declination.

This is not the place to discuss the relative merits of this system and Chandler's, though we probably all regret that there should be two systems in use. But since the early part of each number (Pickering's and Chandler's) is based on the R.A. for 1900, one is convertible into the other to a fair approximation, and thus the table given is useful for Chandler numbers also. It is almost a pity that so useful an index should not be separately printed and more widely distributed. The 'Harvard Annals' are generously distributed, but they naturally cannot reach all individual variable-star workers.

CAN any one tell us whether a similar index exists for the names of minor planets? It would be a troublesome list to keep up to date; but the difficulty of an enterprise is to ardent spirits an inducement rather than a deterrent.

IN Pub. A. S. Pacific, No. 116, Mr. H. D. Curtis gives a most interesting account of "recent changes at the observatory of the D. O. Mills expedition" to the Southern Hemisphere. The daily or nightly climb of 860 feet to the observatory from the homes of the observers a mile away is, we learn with admiration, "a matter of much less bodily strain than would be thought—after one is well hardened to it." But the building of two sleeping-rooms at the observatory, which enables the tired observers to turn in at once after their work, is very gratefully recorded. Since this saves, not the climb up, but only the walk down, there is a special significance in the implied comment. The 36-inch reflector has been differently mounted on new bearings of a frictionless type, and it is pleasant to read of the courteous action of the Anti-Friction Roller Bearing Company, of Los Angeles, in granting free use of their patent to another firm in the interests of science. But perhaps the most important change from a scientific standpoint is the artificial cooling of the mirror at sunset to prevent a slow increase in focal length, which is found to continue for several hours if the mirror is left to cool in the natural way. I must refer to the paper itself for details, but should like to ask the question whether any trouble is experienced from dewing of the mirror when it is cooled in this way so as to be for a time (if I have understood the process properly) cooler than the surrounding air?

[Since writing the above we have received 'Lick Observatory Bulletin No. 122,' where a fuller account is given, containing the following sentence:—" Frost gathers quickly on the cooling coils, but no evidence has been found of any moisture forming on the silver surface, even when the mirror is two or three degrees Centigrade below the temperature in the dome." The appropriate comment would seem to be " Prodigious ! "]

To the welcome news of fresh observatories and equipments mentioned a few months ago, it should have been added that Mr. R. T. A. Innes is to be provided with a 9-inch Grubb refractor. I gather that it is, in part at any rate, a private gift, but doubtless we shall hear in due course what is to be made public on this head.

FROM 'Round the Horn before the Mast,' by Basil Lubbock, p. 308:—

It was my wheel from ten to midnight, and it was rather a case of

" The stars were dim, and thick the night,
The steersman's face by his lamp gleamed white,
From the sails the dew did drip;
Till clomb above the eastern bar
The horned Moon, with one bright star,
Within the nether tip."

Can anyone give the reference to this poem ?

WHETHER our excellent and wonderfully wise friend Mr. Dooley often makes reference to things astronomical my acquaintance with his writings is not sufficient to tell me. But the following instance may be worthy of record; it is from "Mr. Dooley on Things Spiritual " in the *St. James's Gazette* for November 8 :—

All ye've got to do is to believe what ye hear, an' if ye do that enough afther a while we'll hear what ye believe. Ye've got to start in believin' befure ye can find a reason f'r ye'er belief. Our old frind Christopher Columbus hadn't anny good reason f'r believin' that there was anny such a place as America. But he believed it without a reason an' thin wint out an' found it. Th' fellows that discovered th' canals on Mars which other fellows think cud be cured be a good oculist hadn't anny right to think there were canals on Mars. But wan iv thim said: "I wondher if there ar-re canals on Mars; I believe there ar-re. I'll look an' see. Be Hivens, there ar-re." If he'd wondhered an' thin believed about clothes poles he'd 've found thim too. Anny kind iv a fact is proof iv a belief. A firm belief atthracts facts. They come out iv holes in th' ground an' cracks in th' wall to support belief, but they run away fr'm doubt.

I'll niver get anny medal f'r makin' anny man give up his belief. If I see a fellow with a chube on his eye an' hear him hollerin', "Hooray, I've discovered a new planet," I'll be the last man in th' wurruld to brush th' fly off th' end iv th' telescope. I've known people that see ghosts. I didn't see thim, but they did. They cud see ghosts an' I cudden't.

THE following two cuttings from the *Daily Telegraph* of November 7 and November 11 respectively were sent by a kind correspondent:—

AMEER'S TELESCOPE.

Sir Mortimer Durand, speaking at a meeting of the Central Asian Society, yesterday, on the life and times of the late Ameer Abdur Rahman Khan, said the Afghan ruler had a great objection to telegraphs and telephones, and did not much care for railways.

* * * * *

As evidence of the Ameer's interest in Western affairs, Sir Mortimer remarked that he once carefully examined the faces in a picture of legislators in the British Parliament, and tried to decide from them which men could be trusted. The speaker said he never met a man who was so altogether satisfactory to deal with. He was brave, capable, resolute, shrewd, and humorous. but not modest, self-denying, or pitiful. He was keenly interested in big guns, and when someone once showed him a telescope he said: "Oh, blow the Moon! What is the good of the Moon to me? Can't you make a gun of it?"

PLIGHT OF A PLANET.

(From Our Own Correspondent.)

NEW YORK, Sunday.

Saturn's rings are falling off! This alarming announcement was sprung upon us two weeks ago, when the financial crisis was uppermost, and it failed to attract even passing comment. Now the financial situation is less acute Saturn has come to the front again. On behalf of *The Daily Telegraph* I have made a special inquiry amongst several of America's leading astronomical observers, and summing their answers up to-day, I conclude that the rings certainly are falling off. The astronomers do not say so in plain English, but the result of "their study of present phenomena regarding Saturn's rings, as determined micrometrically by the official staff at the Flagstaff, Arizona," leaves no possible room for doubt. Two of the most important rings are disappearing, and apparently for an indefinite period, because, as Professor Lowell, who knows more about the question than any other American, declares in a telegram from Arizona, which I saw to-day, "the condensations are practically permanent and symmetric for both sides of the ring system."

Further analysis shows not only that ring B and the crape ring are travelling outside the general plane, but that their particles are jostling and colliding with each other so successfully that energy is being lost; in fact, to use a pugilistic expression, the rings are being "knocked out" and in danger of falling in upon the planet. While this process is being consummated, Saturn promises to succeed Mars in America as the planet exciting the most popular concern.

Physics

DOES NOT CIRCULATE

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